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MANHATTAN DISTRICT HISTORY

BOOK I - GENERAL [redacted]

VOLUME 4 - AUXILIARY ACTIVITIES

CHAPTER 14 - INVESTIGATION OF MISCELLANEOUS PROCESSES OF SEPARATION OF URANIUM ISOTOPES

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Manhattan District History

Book I – General

Volume 4 – Auxiliary Activities

Chapter 14 – Investigation of Miscellaneous Processes of Separation of Uranium Isotopes

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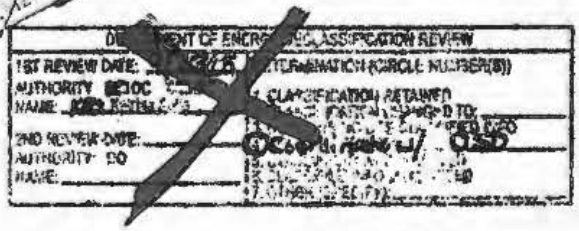
CHAPTER 14 - INVESTIGATION OF MISCELLANEOUS PROCESSES OF SEPARATION OF URANIUM ISOTOPES



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FOREWORD

This Chapter presents brief general accounts of the investigations of a number of miscellaneous processes suggested for separating uranium isotopes. Some of those processes held early promise of success while others, of less promise, were investigated to assure full coverage of the overall separation problem.

As will be indicated herein none of these processes was adopted as a production measure.

This portion of the Manhattan District History dates from the inception of the individual projects, regardless of whether or not they began before Manhattan District control, and, with the exception of the Ionic Centrifuge Method, extends to the project termination.

1 March 1949

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SECTION 1 - THE CENTRIFUGE METHOD

1-1. Purpose - In May 1941 the National Academy Committee, in reporting on their review of the then existing uranium problem, stated in part:- "The separation of the isotopes of uranium can be done in necessary amounts. Several methods are under development, at least two of which seem definitely adequate, and are approaching the stage of practical test. These are the methods of the centrifuge and of diffusion through porous barriers. Other methods are being investigated or need study which may ultimately prove superior, but are now further from the engineering stage."

Later events were such that greater progress was made in other directions and the centrifuge method was ultimately abandoned. It was, nevertheless, determined to be a means by which isotopic separation could be obtained and the purpose of this section of the Manhattan District History is to outline the pertinent accomplishments under that program.

1-2. Scope - The inception of the method and much of the research on centrifugal separation of uranium isotopes preceded the formation of the Manhattan District. The Naval Research Laboratory, Carnegie Institution of Washington, National Defense Research Council, and the

Office of Scientific Research and Development contributed funds toward the development. However, as the project termination was not until 31 January 1944, final control was assumed by the Manhattan District. The work under the overall development was divided among a number of institutions. The University of Virginia conducted extensive experimental research. Columbia University was responsible for theoretical research. The Westinghouse Electric and Manufacturing Company developed the centrifuge machine at their Research Laboratory and constructed production model parts at another of their subsidiary organizations. The Standard Oil Development Company developed, erected and operated a pilot plant. Activities of each of the participants will be covered by the following.

1-3. Early History of the Centrifuge Process - The account as here presented is a condensation of historical information provided by Professor J. W. Beams in his report of 7 December 1946 on the subject matter (App. B-1).

The possibilities of separating isotopes by centrifuging was first suggested by F. A. Lindemann and F. W. Aston in 1919. They also worked out the equilibrium theory for separation in an ideal gas and in an ideal incompressible liquid. Following the above work of Lindemann and Aston, the theory was critically discussed and extended by R. S. Mulliken, S. Chapmann, and W. D. Harkins. Mulliken also investigated theoretically centrifugal separation in ideal liquid isotopic mixtures and suggested the so called "evaporative centrifuge" method. That method consisted of drawing out vapor from the axis of a hollow spinning rotor containing the liquid in its periphery. In this method the

separation takes place in the vapor as it diffuses from the periphery through the centrifugal field to the axial point of withdrawal.

Encouraged by theory which showed that considerable separation of isotopes should be obtained, various attempts were made to separate different isotopes in specially constructed centrifuges. Unfortunately, each of these early attempts proved to be unsuccessful, ~~due~~ probably because of convection, and the method was abandoned as impracticable. With the development of the convection free vacuum type ultra-centrifuge in 1934, the method was tried again and results were successful. In most of the early successful experiments the evaporative centrifuge method was employed to concentrate Cl_{37} and Cl_{35} isotopes of chlorine in CCl_4. Bromine isotopes also were concentrated. It was found that considerable separation could be obtained and that the results were in good agreement with the theory when equilibrium conditions were approximately maintained in the centrifuge. The theory was further tested at dry ice temperature, with different rotor speeds, and found to hold. G. H. Wilson and A. Bromley further developed the theory, but it remained for H. C. Urey, K. Cohen and their collaborators to work out the general mathematical theory and to indicate theoretically the most efficient way to go about the separation of isotopes by centrifuging.

In addition to the evaporative centrifuge method several other schemes were tried. From elementary theory it was clear that the rate at which separation could take place in a centrifuge rotor was proportional to the length, or depth, of the rotor. Accordingly, procedures for spinning tubular rotors were developed for the evaporative centrifuge and several other methods.

The so-called "flow through", "cream separator" or "concurrent" method utilized the tubular vacuum type centrifuge. Operation was performed with ethyl chloride vapor, at about 300°A, flowing through the hollow shaft into the top of an alloy steel, 3" I.D. by 14" long, tubular rotor (containing end baffles) which was spinning at 1060 r.p.s. Two equal fractions were collected at the lower end of the spinning tube, one near the axis of rotation, and the other near the periphery. The observed separation factor between the two samples was 1.025 with a continuous rate of flow of 2 grams per minute. Also, with the same apparatus at various rates of flow up to 2 liters per minute, a large number of observations were made on the separation of gaseous mixtures of N_2 and O_2 and N_2 and CO_2.

The "distillation" or "cascade" method, first suggested by Urey, was also tried, using the above type of tubular rotor. About 65 cc. of liquid CCl_4 were placed in the 3" I.D. by 14" long tubular rotor. The tube, which contained radial baffles, was spun to 1060 r.p.s. and then evacuated and sealed. The upper end of the spinning tube was surrounded by a copper coil carrying cooling liquid. H_2 at about 3 mm. Hg pressure was introduced into the vacuum chamber surrounding the rotor to conduct heat to it. This arrangement made the liquid evaporate in the lower warm part of the rotor and condense in the upper cool part, thus, providing for a vapor circulation upward and a liquid flow down the periphery. Small samples, collected at the top and bottom of the rotor after 15 hours centrifuging, were analyzed in the mass spectrometer. Although results were erratic in some cases, the separations observed were slightly larger than could be accounted for by a simple

centrifuging process and evidenced that some cascading was taking place.

In further experiments at the University of Virginia a duralumin tube 1-1/8" I.D. by 50 cm. long, spinning at 2300 r.p.s., in air at atmospheric pressure was used. The tube was first evacuated and from 5 to 6 cc. of CCl_4 was distilled into it. The tube was then sealed and spun to full speed. Tap water (about 20°C) was sprayed on the top of the tube to cool it while the lower end of the tube was heated by air friction. After spinning for about two hours a small sample (about 1 cc) of CCl_4 was pumped out through the hollow tube shaft and condensed. The Urey cascading process was shown to exist by some of the experiments, as the separation obtained was between 4 and 5 times that possible under a single centrifuging. However, in practice, this method did not offer the ease of accomplishment or appear to promise large scale separation of isotopes.

Another method of producing circulation in the spinning tubular rotor was tried when the material centrifuged was in the gaseous state or in the liquid state. A small rod of resistance material, placed along the axis of the spinning tube, was heated by an electric current connected through the top and bottom shafts. The decrease in density, due to the increase in temperature near the axis, was intended to cause the gas (or liquid) to rise along the axis, and move downward along the cooler rotor wall in a manner similar to the Clausius and Dickel Thermal Diffusion Experiment. Primarily this circulation is caused by the gravitational field of the earth and is opposed by the stabilizing influence of the centrifugal field (to be

discussed later) where the gas moves radially at the upper and lower ends of the rotor. The experimental results were not conclusive. Also, some difficulty was encountered in supporting the heating rod, or tube, because of its natural vibration. This experimental difficulty was overcome sometime later, but the early experiment was dropped as it seemed at the time to be inferior to other centrifuge methods for production purposes.

Experiments were also tried in which a "mechanical elevator", or screw, was placed along the axis of a tubular centrifuge to carry the gas, or liquid, up along the axis of the centrifuge. Centrifugal force was then expected to cause the elevated material to flow back down along the wall of the tube. Unfortunately, the experiments were never completed.

H. C. Pollock and K. H. Kingdon used a circulating evaporative centrifuge method for the concentration of the tin isotopes in $SnCl_4$, but the method was not extended.

In 1940 R. Gunn suggested an ingenious method of isotope separation in which the sedimentation produced by a high centrifugal field was opposed by the mobility of the ions under an electric field. He also worked out a detailed theory from which he could predict the separation of the U-235 and U-238 isotopes. It was believed that extensive separation should be obtained.

1-4. Research at the University of Virginia - Professor J. W. Beams and his associates of the University of Virginia had been prominent in the centrifuge method of isotopic separation since its inception. In March 1939, shortly after the announcement of uranium

fission, Beams and Professor L. J. Haddy became interested in the centrifuge purification of U-235. The centrifuge method seemed well suited for that purification as the separation factor by such means depended upon the difference in the exponent of the isotope masses rather than upon their absolute value. Furthermore, the separation was independent of the state of combination of the element and the factor did not decrease with an increase of concentration of the rare isotope.

A study of literature concerning its chemical and physical properties indicated that uranium hexafluoride possessed possibilities for use in the centrifuge process. It was a liquid at 59° C with a vapor pressure of two atmospheres. On the other hand, UF_6 was not available commercially and as far as could be learned only a small amount of the material had been made at that time (1939). Shortly after preliminary centrifuge work was under way at Virginia, Dr. Ross Gunn of the Naval Research Laboratory, who was actively interested in the problem because of its military possibilities, agreed to try to procure enough UF_6 to proceed with tests. It, however, soon became apparent that the procurement of UF_6, in quantities sufficient for the proposed experiments, would take considerable time - it actually required about one year. In the meantime Haddy undertook to make a small amount of UF_6 in order to study its physical and chemical properties.

While waiting for enough UF_6 to start centrifuge experiments it was considered advantageous for the University of Virginia to attempt to separate isotopes of available material for determination of the most efficient centrifuge method to be followed. They had suggested several

cascade processes, using long tubular rotors; also, previous experiments on the separation of the chlorine isotopes in CCl_4, and the separation of N_2-CO_2 gaseous mixtures, had demonstrated the advantage of a long tubular rotor.

The desired operation of spinning tubular rotors, many times longer than their diameter, to high speed presented problems. First, such rotors pass through "critical" frequencies before reaching the speed required for operation. At the first critical speed they vibrate like the fundamental of a vibrating rod and at the second critical speed, depending upon the mounting, they vibrate like the first harmonic, etc. In previous work at Virginia alloy steel tubular rotors 3" I.D. by 14" long had been spun well above their first critical frequency. It had been found that, in order to spin them through the critical frequency, special damping of the bearings was required, but that when the speed exceeded the critical frequency the tube spun stably and smoothly for long periods without any sign of fatigue. Nevertheless, it was not at all certain that tubes with much greater length to diameter ratio could pass through the critical frequencies and reach the required speed. Also, it was a debated question whether or not the tubes would spin stably without fatigue above the second or higher critical frequencies. After considerable research on damped bearings it was subsequently found that long tubes could be spun through as many critical frequencies as desired and that they operated stably at high speeds.

At the early stage of the work on centrifuge separation of uranium isotopes the University of Virginia received a grant from the Naval Research Laboratory. That assistance was followed in April, 1940,

by a grant in aid from the Carnegie Institution of Washington. This latter grant, together with a Naval Research Laboratory contract, made it possible to direct the major efforts of the Physics Staff and graduate students at the University into this problem during the summer of 1940. Following the Naval Research Laboratory contract the subject work was conducted under contracts OEMsr-140 and OEMsr-398 between the OSRD and the University of Virginia. These contracts were respectively effective 1 August 1941 and 1 March 1942.

In February, 1940, Gunn made approximately 6 grams of UF_6 available to the University of Virginia. That material was the first sufficient quantity for operation of the centrifuge. It was subjected to isotopic separation by the evaporative centrifuge method for which a theoretical separation factor of 1.08, or an 8% change in isotopic ratio, applied. The UF_6 was successfully centrifuged without appreciable loss, or deterioration, and the samples were sealed in glass capsules. At that time (February 1940) the University of Virginia had no means of analysing the samples. They were sent away to be analyzed but apparently that analysis was never performed. However, the feasibility of handling UF_6 in the centrifuge was very definitely demonstrated.

About that time active work on the centrifuge method was started at Columbia University. Urey had suggested several methods for attacking the problem and his group, led by Cohen, worked out a general separation theory which subsequently prompted a conclusion that the refluxing counter flow method, using long tubular centrifuges, was the most efficient for large scale separation.

By the middle of 1940 developments at the University of

Virginia had reached the point where tubes of various sizes with lengths many times their diameter were spun up to their bursting speeds. Consequently, a systematic set of experiments was undertaken to test the efficiencies of the following methods.

(1) The "evaporative" method.

(2) The "distillation" method.

(3) The "flow through" or "con-current" method.

(4) The "counter-current" method.

UF_6 was unavailable for these tests until 1941 and many of them were made with compounds of chlorine, bromine, and mixtures of N_2 and H_2 and N_2 and CO_2.

The evaporative centrifuge method was used to carefully check the centrifuge theory and to make several concentrated samples. In December, 1941, two samples of 1 gram each were obtained whose difference in isotopic ratios, as measured by A. O. Nier, was about 40 percent.

The distillation method was abandoned after early tests because of its inferiority to other methods. It was, however, found to give cascading effects.

Data in good agreement with Cohen's theory was obtained with the flow through, or con-current, method and with the counter-current method.

The counter-current method was the one finally decided upon for pilot plant operation.

During the latter part of the activity at Virginia effort was made to get data which would permit an estimate of the performance of a large number of centrifuges in a cascade, or production, plant.

That activity was carried out in close cooperation with the Standard Oil Development Company. Also, through the assistance of the Standard Oil Development Company, the University of Virginia was able to obtain and operate an 8.35" O.D., 7.35" I.D., by 136" long, forged duralumin tubular centrifuge rotor for use in counter-current refluxing centrifuge experiments.

The foregoing presents a brief general account of the centrifuge experimental work at the University of Virginia. Details of that work which include general comments, description of apparatus, operation information, and records of experimental results for the evaporative method, flow-through or con-current method, counter-current flow method and the counter-current refluxing method, are provided by the contractor's report (App. B-1).

1-5. Theoretical Research at Columbia University - The OSRD awarded contract OEMsr-192 (continuation of contract NI73s-3974) to Columbia University for theoretical research on the centrifugal separation of uranium isotopes. As referred to by the foregoing, Professor Beams, of the University of Virginia, had developed high speed centrifuges which operated by the simple flow-through process and by the evaporative process. Under the flow-through process a gas entering at one end of the centrifuge was segregated, in its flow through the machine, to its heavier and lighter fractions at the other end. The evaporative process operated with a liquid supply which was evaporated from the centrifuge leaving a residue of heavy isotopic concentration.

The work under contract OEMsr-192 initially concerned the use of a counter-current flow of liquid and gas. The liquid was proposed

to be evaporated at one end of the centrifuge rotor and the gas condensed to a liquid at the other end of the rotor. The operation was somewhat similar to a distillation column. Later, because of the great stability of the flow of gases and because of the strong field produced by the centrifuge, the initial proposal was modified to the counter-current flow of two gases.

Extensive consideration was given to the fractioning at centrifuge. Estimates were made of the overall rotor length to produce the desired concentration. This work revealed the magnitude of the separation problem and it also demonstrated the advantages of the counter-current centrifuge method as compared with the flow-through centrifuge. Outstanding among those advantages were the use of smaller shafts (because of the reduced flow from centrifuge to centrifuge) and a somewhat shorter overall length centrifuge required for a given production, or separation.

Centrifuge developments which had been, or were, made during the early part of the subject contract confirmed the magnitude of the problem. Contacts had been made with the Westinghouse Company for the development of centrifuge machines. Those contacts resulted in a centrifuge development sub-contract, under Columbia contract M173s-3974, which is described in a following portion of this history.

The general properties of cascades as applied to separation problems were first worked out in connection with the centrifuge method. In 1939, Urey had suggested a centrifugal isotope separator which could be used continuously in a cascade. In 1941, Cohen made a detailed analysis of the Urey unit (the fractional distilling centrifuge) operating

of UF_6. Since that time various other schemes and modifications of the original idea were proposed and analyzed. Numerical estimates (for example, of the size of the plant) which were made served to orient the development in the direction of large scale production. Comparisons were made with the following types of centrifuge performance:

(1) The fractional distilling centrifuge as suggested by Urey.

(2) The fractional distilling centrifuge with baffles.

(3) The counter-current gaseous flow centrifuge.

It seemed unlikely that flow distillation could be established in a centrifuge process. The all metal construction of the rotor made it difficult to localize evaporation and condensation, and apparently circulation from cap to cap broke down in localized currents. Another important item from the standpoint of cascade operation, even if circulation of the correct sort could be established, concerned the system of control. Control of the fractional distillation process would be almost impossible and the above types (1) and (2) did not appear to be realizable.

The fundamental difficulty in operation of a centrifuge containing material at its boiling point is that any temperature irregularity becomes a source of trouble. Accordingly consideration was given to an all gaseous counter-current process wherein circulation was produced by pumps which were outside of the rotors. The principal numerical result was that it was established to be within the range of technical ability to produce 1 Kg./day of U-235 by use of 24,000 to 25,000 centrifuge units.

Details of the subject research and results obtained at Columbia University are provided by reports referred to by Appendices 3-2 to 3-12 inclusive.

1-6. Development by Westinghouse Electric and Manufacturing Company - The following general account of the development of centrifugal gas separators is essentially a quotation of the historical portion of an overall report prepared by the Westinghouse Research Laboratories. Details of that development are also provided by that report and reference is made in that respect to Appendix 3-13.

The problem of developing a suitable centrifuge for separation of uranium isotopes was first considered by the Westinghouse Research Laboratories in August, 1940, as the result of an inquiry by Columbia University. Approximate requirements, given early in December of that year, indicated a rotor diameter of about 8 inches, a length of two to three feet, and a speed of 500 r.p.s. Early discussions were almost wholly confined to the problems of supporting and spinning the rotor cylinder at the high rate of speed.

Early in 1941 a contract was received by the Westinghouse Company from Columbia University for the design of a centrifuge to separate uranium isotopes. The proposed work concerned the bowl design, its support and the high-speed drive. Other major problems, namely the development of pumps and entry and exit shaft seals for the process gas, were retained by Columbia University, although, later, this work was turned over to Westinghouse. The possibility of large scale production of centrifuge units entered into the consideration and influenced many phases of the development.

It was desirable that important design principles and detail improvements revealed by centrifuge research at the University of Virginia and Columbia University be incorporated in the apparatus design. Hence, rigid specifications at that time were not desirable and were not drawn up. As a starting point in the dynamical design it was generally agreed that the rotor bowl geometry be such that it could be treated as a rigid body, that is to say, the frequency of the lowest natural bending node of vibration of the bowl as a free body was to be higher than the running speed. This consideration for an 8 inch diameter steel or aluminum bowl, operating at 500 r.p.s., limited the length to about 42 inches.

The centrifuge design for Columbia University was completed about December, 1941, and shortly thereafter the Office of Scientific Research and Development contracted, under OEMsr-415 and 485, for a full size dynamical model centrifuge and 24 production units to be built by the Westinghouse Company. The production units were for installation in a centrifuge pilot plant. Construction of the dynamical model began in January, 1942, and production of detailed parts for the pilot plant machines by the Westinghouse Electric Elevator Company began in March, 1942.

Before tests and development work on the above model were completed, and before completion of the first of two production units ultimately delivered to the pilot plant, overall project considerations made it apparent that the bowl length of the centrifuge should be increased as much as possible. A long bowl cylinder length was established at 132 inches. That length presented the problem of bending vibration of the bowl itself. The result of detailed investigations of

bearings, damping, material strength, and parts design carried out in the development of the 42-inch length machine were naturally applicable to the 132-inch centrifuge and in these respects the work was simplified.

Two 42-inch centrifuges of 7.2 inches inside bowl diameter and 8.2 inches outside bowl diameter, with rated speed of 470 r.p.s, were delivered to the centrifuge pilot plant in May and September of 1943. These machines operated under pilot plant performance. The overall length of each unit was 123 inches and each machine weighed 1000 pounds. A dynamic model of the 132-inch centrifuge, with bowl diameters as shown for the 42-inch machine, was assembled and tested in December, 1943. The overall length of the 132-inch machine was 188 inches and the total weight was 3200 pounds. This machine was wrecked in test; however, its performance indicated that successful design and operation of a centrifuge of such length were probably possible.

1-7. Pilot Plant Development and Operation by the Standard Oil Development Company - In January, 1942, the Standard Oil Development Company entered into a program under the Office of Scientific Research and Development for the concentration of uranium isotopes by the centrifuge method. That program, which terminated 31 January 1944, was covered by the following contracts between OSRD and the Standard Oil Development Company.

OEMsr-567, effective 9 January 1942.

OEMsr-925, effective 1 January 1943.

OEMsr-926, effective 1 January 1943.

The contractor's work under the above contracts included principal phases as here shown:- (1) cooperation with the SAM Laboratories in making

Engineering analysis of the centrifuge method of gas separation; (2) cooperation with the Westinghouse Research Laboratories in the development of a suitable centrifuge to operate on process gas; (3) design and construction of a pilot plant to test the centrifuge method as applied to the process gas; (4) operation of the pilot plant to obtain separation data under a variety of conditions and to test the performance of the Westinghouse centrifuge; (5) an engineering survey of the application of the centrifuge method to a large scale plant; and, (6) correlation of the experimental separation program carried out in the corresponding centrifuge project of the University of Virginia.

As indicated by this history, the theoretical basis and the experimental research for the overall project had been and was being provided respectively by Columbia University and the University of Virginia. The design and development of the centrifuge proper was assigned to the Westinghouse Company. It had been proposed that the centrifuge be operated in a counter-current manner, and considerable progress had been made in features of operating a cascade of counter-current centrifuges on process gas. Under the Standard Oil Development Company contracts the engineering aspects of that process were further developed, with particular reference to a cascade suitable for production of 1 Kg. of the desired product, in 90% concentration, per day. The pilot plant contractor's analysis also included working out the most appropriate methods for testing the theory of cascade counter-current gas separation by means of a limited number of machines in a pilot plant.

Centrifugal separation of the process gas had been recognized at the outset as requiring, among other things:- (1) the use of

corrosion-proof metals; (2) prevention of contact of the process gas with reactive materials such as lubricating oil; (3) the use of absolute pressures in the order of 10 to 20 mm. Hg. (low enough to prevent condensation of process gas at the periphery of the bowl, at permissible bowl temperatures, and in the piping at room temperatures); and, (4) the provision of an exceptionally tight process system to prevent loss of process gas (either by out-leakage of process gas or by in-leakage of moist air).

The pilot plant design and construction work undertaken at the beginning of the program provided for the installation of 24 Westinghouse short-bowl (36") centrifuges. The plant, known as the Gas Separation Pilot Plant, was located at the Standard Oil Bayway Refinery, Linden, N. J. Provision was made at the refinery for:- (1) an electrical drive system for the centrifuges; (2) the required centrifuge auxiliaries such as lubricating oil, circulating water, nitrogen seal gas, and hydrogen casing gas; (3) a process gas circulating system; and, (4) a refrigerating system for chilling the process receiving traps.

At an executive meeting of the S-1 Committee, on 23 October 1942, Mr. Z. G. Deutsch, of the Standard Oil Development Company, presented a paper concerning the use of hexafluoride as a feed material for the centrifuge process. He also presented another paper, in abstract, outlining a full-scale production plant as it was then foreseen (App. A-1 and A-2). A further analysis of the centrifuge method of concentrating uranium isotopes was provided by the pilot plant contractor subsequent to a conference on 24 February 1943 between representatives of the Kellex Corporation, the Westinghouse Research Laboratories and

the Standard Oil Development Company (App. A-3).

In November, 1942, decision was made by the OSRD committee in charge of the centrifuge project to change the centrifuge pilot plant from 24 to a single centrifuge basis. This was for the purpose of obtaining, as promptly as possible, fundamental single-machine separation data needed as a first step in testing the counter-current gas separation theory. The state of pilot plant construction and equipment procurement at that time was such that the change to the single machine basis involved only minor modification in the plans for the electrical, auxiliary, process gas, and refrigeration systems. Construction of most of the auxiliaries, except connecting piping, was continued on a multi-machine basis.

Development of the centrifuge for use in the pilot plant had been, and was being, carried out by the Westinghouse Research Laboratory during the early part of 1943. The first of the so-called short bowl machines was delivered to the pilot plant on 29 May 1943 with a temporary bowl fabricated from a duralumin forging. Assembly of the machine and its pipe connections was completed and various running and tightness tests were carried out in the period up to 16 July. At the later date the permanent extruded (full strength) bowl became available for installation. The completed machine was employed for about five weeks of experimental operation commencing 18 August. Following this period a second machine with improved damper construction and corrosion-proof (monel) shafts was installed in place of the first unit. Operation of the second machine was initiated 9 October and continued at various intervals throughout 1943.

Although some mechanical difficulties, usually involving damaged bearings, were encountered with both of the centrifuges as installed at the pilot plant, nevertheless, those faults were correctable. After correction, run failures due to faulty rotating parts on either machine were practically non-existent.

The engineering analysis of a large plant employing long bowl centrifuges, first made at the outset of the Standard Oil Development Company program, was revised in very general form in December, 1943, in the light of the more attractive fundamental separation data which had become available. Preliminary plans were also made at this time for converting the centrifuge pilot plant to a multi-machine basis to test cascade operation.

The handling of process gas in a centrifuge system required the development of various special instruments and other equipment. Under this project, development work was carried out on a number of these items, including principally:- (1) a trap for continuously separating process gas from nitrogen seal gas; (2) low pressure-drop thermal flowmeters; and, (3) bellows-type valves meeting extreme tightness specifications.

The centrifuge pilot plant project also included a substantial amount of laboratory investigations relating principally to determining the corrosive characteristics of various metals, rubbers, plastics and liquids when used with process gas. Laboratory work also included operation of a Nier mass-spectrometer for analysing process gas samples produced by the pilot plant operation.

The foregoing general account of the centrifuge pilot plant project has, in the main, been taken from the Standard Oil Development Company report on the Gas Centrifuge Development Project (App. B-14). Reference is made to that report and the details contained therein if more complete information is desired concerning the overall centrifuge pilot plant project.

1-8. Discontinuance of the Centrifuge Method of Isotopic Separation - S-1 Committee meetings, held 10 and 11 September 1943, resulted in a vote to permit the University of Virginia contract to run to its completion date; that was, not to stop the centrifuge work at that location at that time, and to have the Standard Oil Development Company continue to spin the short bowl centrifuge for an aggregate of 4 months at as small expense as possible. Dr. N. V. Murphree, one of the Committee members, and also an official of the Standard Oil Development Company, indicated certain additional costs which would be involved to:- (1) complete development of the short and long bowl machines; (2) continue pilot plant operation with the above machines; and, (3) cover the possible continuation of centrifuge research at the University of Virginia. A vote to determine authorization of the directly above resulted in a tie. Committee members Doctors Murphree, Compton and Lawrence were favorable to the full program and Doctors Conant, Briggs and Urey favored the curtailed program previously voted. It was determined that final decision for the S-1 Committee was to be made by Dr. V. Bush, Director OSRD, without benefit of any recommendation from the committee (App. A-11), but the situation changed as the result of later developments.

Through his letter of 6 December 1943 to the other members of the S-1 Committee, Murphree provided a summation of the centrifuge project as it then existed (App. A-4 and B-15). That letter, and the report

transmitted therewith, primarily concerned results obtained through pilot plant operation and referred to an existing plan to discontinue those operations as of 31 December 1943. The letter indicated that satisfactory results were being obtained and recommended that the overall project be carefully reviewed by the OSRD, or the Army, prior to bandonment.

Letters of 7 December and 11 December 1943 from Urey, respectively to General Groves and Conant, contain his views as to the possible attractiveness of the centrifuge method (App. A-5 and A-6).

From an independent investigation made at the direction of General Groves it did not appear that the centrifuge method could catch up with the then existing electromagnetic and gas diffusion methods of uranium isotope separation. It did appear that extensive, if not prohibitive, facilities would be required for the wartime construction of the proposed centrifuge plant.

On 18 December 1943 General Groves communicated with Conant and invited his attention to:- (1) the condition that the S-1 Committee had not recommended that the centrifuge method be carried forward; (2) the appearance that it would be impossible to complete the centrifuge plant engineering and construction in time to be of value in the war; and, (3) the fact that more than one method of producing the desired product had already been embarked upon. Conant's views from a scientific standpoint were requested (App. A-7).

Dr. R. C. Tolman, who had inspected the centrifuge pilot plant on 15 December 1943 informed General Groves, by his letter of 20 December, of his views on the merit of continuing the centrifuge method (App. A-8).

Acting upon the request of Conant, Murphree replied to him on 22 December and presented a rough time schedule for large centrifugal plants (App. A-9). Under the same date Murphree also wrote to Conant outlining the status of the centrifuge process and indicated further experiments which would be required if the centrifuge development program was to be continued. (App. A-10).

Conant's reply of 28 December to General Groves' letter of 18 December stated that arrangements had been made for comment of the individual members of the S-1 Committee in lieu of a formal vote of the Committee. Conant stated his personal opinion regarding the centrifuge program to be:- (1) that in consideration of the time schedule, as provided by Murphree, it was impossible for the centrifuge method to be brought into the then present program except as an additional insurance against failure of the three methods being pushed; (2) that the United States was then spending all of the money, time, and material then justified on the overall separation program; and, (3) that an insurance program which would materialize at such a late date would be entirely unjustified (App. A-11).

Murphree presented his opinion concerning the continuation of the centrifuge project to General Groves on 31 December. He reasoned that two methods of obtaining the final product appeared to be sufficient and that the electromagnetic method had progressed too far to warrant replacement. It was suggested that the study of the feasibility of replacing the diffusion method by the centrifuge method be determined by a committee to be appointed by General Groves (App. A-12).

Lawrence informed General Groves on 1 January 1944 of his

recommendation that the centrifuge program be terminated (App. A-13).

Urey, in his letter of 3 January to Conant stated his favorable opinion of the centrifuge method (App. A-14). Urey also informed General Groves, on 3 January, of his opinion that a committee should be appointed to study the desirability of substituting the centrifuge method for the diffusion method of isotope separation (App. A-15).

The 3 January comment of Briggs to General Groves was that procedure with the centrifuge method should be dependent upon General Groves' opinion regarding the duration of the war (App. A-16).

On 12 January Compton indicated his opinion in a letter to Conant (copy to General Groves) that efforts should be concentrated on methods already in hand and that the centrifuge production plant should not be undertaken (App. A-17).

For reasons which have been indicated by the foregoing General Groves informed Conant on 19 January 1944 that further extension of the centrifuge project was not justified at that time (App. A-18).

Shortly after the above exchange of correspondence work upon the centrifuge method of separating uranium isotopes was brought to a termination. While at that time there was still some question regarding the suitability of the other methods of separating the desired product, nevertheless, later events, particularly in regard to the successful production obtained by the diffusion method, have proven the wisdom of the decision made.

SECTION 2 - THE ISOTRON METHOD

2-1. General - During the late summer and fall of 1941 Professor H. D. Smyth, as a member of the NDRC Uranium Section, became particularly interested in electromagnetic methods of separating uranium isotopes. At that time Professor E. O. Lawrence was proceeding with his work on the "calutron" and obtaining evidence of promising results (see Book V). One of the features of the "calutron" consisted of an essentially one-dimension slit type of source. In contrast to that feature it appeared that an extended ion source would possess obvious advantages. This was discussed between Smyth and Dr. R. R. Wilson and the latter proposed a scheme by which it was believed that a plane source of considerable area could be used. The proposal was based upon the "bunching" of ions in a manner similar to that done by a Klystron high frequency oscillator. Because of that similarity the scheme was at first referred to as the "Klystron" method of separation, but later, as a security measure, it was identified as the "isotron" method.

From the above inception the isotron proposal developed into a project at Princeton University, under OSRD contract OEMsr-279, at an approximate cost of $480,000. The contract was effective on 22 December 1941 and was terminated by OSRD on 15 February 1943.

2-2. Theory - The theory of isotopic separation by the isotron method is based upon ions from a suitable source being accelerated into a field free region; first, by a large constant voltage field, and, second, by an alternating voltage field, preferably of saw tooth wave form. Ions going through the alternating field at the beginning of

a cycle would not be further accelerated, but those reaching it later in the cycle would get an increment of kinetic energy proportional to the instantaneous value of the alternating voltage. The resulting increase in velocity of the later ions would be such as to cause them to catch up with those that had passed earlier into the field free space, i.e. the ions would "bunch". The foregoing was expected to result in one location, within a suitable machine tube, which would be reached by all ions of the same mass which entered the alternating field during the cycle. However, as ions of different mass would be accelerated at different velocities they would reach the bunching location at different times. Thus, assuming ions of two different masses being accelerated from the source, by choosing the proper value of period oscillation, accelerating voltage and distance to the bunching point, a bunch of one mass ions can be made to occur midway in the time interval between bunches of the other mass ions. Separation of the bunches can then be achieved by setting up deflecting plates to divert the ion masses in different directions of travel. As the bunching effect described is along the direction of motion of the ions it therefore imposes no limitation on the cross-sectional area of the beam or the source.

2-3. Research and Development - From a theoretical standpoint there appeared to be no obvious impossibilities in the isotron method; nevertheless, it was quite evident that many difficulties were probable in its development to a large scale production plant. Some of those difficulties were applicable to all electromagnetic methods of separation, some applied in particular to the isotron method, but, fortunately, some which were very important in other methods were of little consequence

in the subject method.

a. For any electromagnetic method to succeed it was recognized that a powerful and adequate source of ions must be available. Outstanding requirements for such a source were reliability, purity and efficiency both as to material and power. When work on the "isotron" method was started the only sources of uranium ions which had been developed successfully were a gas discharge in hexafluoride and electron-controlled arcs in chloride vapor. Early adaptations and developments for the isotron method resulted in two sources, either of which appeared to be usable, but neither of them was entirely satisfactory. Those sources were as follows:

(1) An arc was established in vacuum between electrodes, to be described, and the ions were drawn out of the plasma of the arc. Innumerable shapes, sizes and materials were tried but, in each case, the uranium eventually destroyed or severely corroded some part of the source apparatus. One of the first electrode materials tried was tungsten. Good sources were established but they deteriorated because molten uranium alloyed with tungsten and, when present in sufficient quantity, gradually ate through it. It was, however, observed that if a small quantity of uranium was used it would evaporate as a vapor without causing serious damage to the tungsten. A source was developed in which a small piece of metallic uranium was dropped on the tungsten rod which formed the anode of a hot

cathode arc. When the uranium was exhausted the potential drop across the arc increased and activated a mechanism to deposit another piece of uranium metal. This supplied more vapor and restored the arc to normal. The principal advantage of this source was the purity of ions obtained from it.

(2) A hot cathode arc in uranium chloride had been extensively used at Berkeley and was the subject of considerable work at Princeton. It provided large currents, ran steadily and served as a thoroughly usable source. Essentially it consisted of a boiler, containing the chloride at a controllable temperature, and an arc chamber into which the vapor diffused. The chief difficulty of this source concerned the purity of the ions obtained.

Hot cathode arcs in metallic vapor were used for several months on each of the experimental "isotrons" placed in operation. An automatic loader for renewing the supply of uranium was devised and worked satisfactorily. The life of the tungsten anode was found to be limited in the order of 6 to 12 hours but it could be easily renewed. Work was also done on a continuous feed mechanisms for both the uranium and the tungsten. That continuous feed gave promise of successful operation.

b. At the beginning of the project it was realized that space charge might be a limiting factor in the isotron operation. It was considered that such effects might be encountered in a number of ways. Space charge could limit the ion current drawn from the source.

Through space charge influence on the shape of the arc plasma difficulty might exist in obtaining proper focus of the ion beam. Space charge might affect the formation of bunches, also it could cause the ions to spread out transversely. Results of early experiments prompted an assumption that space charge effects would not be as serious as at first feared. However, later experimental operation indicated the initial assumption not to be wholly correct. Certain difficulties then encountered caused a very definite suspicion that space charge, while not spreading the beam as a whole, was, nevertheless, affecting bunching and keeping the separation factor down. Investigations in this respect led to discovery of "second order" bunching. It was established that because of space charge there was a critical value of current density above which "first order" bunching fell off rapidly and high values of separation were not likely. Various operations with two of the experimental isotrons confirmed this relation. The theoretical formula for the limiting current density suggested raising the voltage and shortening the isotron tube as a remedial measure. Raising the voltage required a change in frequency and an increase in voltage on both buncher and analyzer. But by certain changes in the phase of collection, it was possible to shorten the tube without altering appreciably the voltage or frequency. When that correction was made on one of the experimental units an increase in usable current density was obtained. The next step was obviously to go to higher voltages and frequencies and a still shorter tube. The combination of higher voltages and higher frequencies would no doubt have presented additional problems in development of a production isotron;

however, it was believed that those problems could have been solved. Unfortunately at that time the impending termination of the overall isotron work made it impossible to adequately redesign or reconstruct either the buncher or the analyzer. As an alternative remedial measure, in the event that a considerable decrease in the tube length and increase in the voltage could not be obtained, it was proposed that steps be taken to neutralize the space charge or that higher order bunching be established. These last-mentioned possibilities were based upon theory only and were not experimentally explored.

c. Reference has been made in paragraph 2-2 to the preference for a saw tooth wave form for the accelerating voltage field. Actually such a wave form was not obtained and various approximations had to be considered. It was, of course, found possible to obtain a degree of separation of isotopes with a normal sine wave if ions accelerated in part of the cycle were discarded. Therefore a determination was required between efficiency and the degree of separation. The efficiency related to the used fraction of one cycle of the accelerating alternating current. The separation of the product was defined as the factor by which the ratio of heavy to light isotopes was decreased. A wave form made up of a sine wave in combination with a number of harmonics appeared to provide a suitable substitute for the ideal saw tooth form. Cooperation was obtained from the Radiation Laboratory at M.I.T. in constructing saw tooth wave form oscillators. One such device operated by successively charging a condenser through a resistor at a steady rate nd then discharging the condenser through a radio tube upon whose grid a sharp

positive pulse was applied. Another oscillator synthesized the desired wave form by adding harmonic sine waves together.

2-4. Experimental Units - The first experimental isotron was set up in January, 1942. The unit consisted of a brass tube 3 m. long and 11 cm. in diameter. The tube was in several sections and had ports spaced every 50 cm. for the purposes of observation, insertion of electrodes, and soforth. The ion source, accelerating electrodes, and bunching electrodes were at one end of the tube with limiting diaphragms, deflecting plates, and detecting devices at the other end of the tube. Auxiliary equipment, such as pumps, high voltage source, arc power source, amplifiers, and oscillators had either been constructed or adapted from equipment available in the laboratory. Initial operation of the first isotron was simplified by the substitution of lithium for uranium as the material to be separated. That substitution had two advantages. First, a plentiful supply of Li^+ ions could be obtained by heating spodumene, thus simplifying the source problem. Second, the isotopes of lithium differ in mass by nearly 15 percent instead of the approximate 1.2 percent for uranium; also the abundance ratio was about 12 to 1 instead of 140 to 1. The operation was further simplified by inserting beam limiting diaphragms near the receiving end of the tube. A high frequency sine wave was applied to the deflection plates. The buncher voltage was applied and its phase varied relative to that of the deflecting voltage. Under operation it was observed that the Li_7^+ ions were deflected to one side and the Li_6^+ ion bunches were deflected to the other side. The current for the first operation was minute - of the order of a microampere - but it was demonstfated that the

method was workable.

Necessary refinements were made to the unit for the separation of ions differing in mass by only 1.2 percent, and early in April, 1942, experiments with uranium were carried out in a manner essentially as had been performed with lithium. Again, success was attained in the experiment, thus proving that the method was workable for uranium isotopes. In the latter experiment larger currents were employed but they were still relatively small for any practicable purpose.

The next objective was to get a larger current down the isotron tube and to analyze the whole current in the tube instead of diaphragming out all but a small part of it. The tube and source conditions were altered until currents in the order of a milliampere could be brought down the tube. The problem of analyzing ions from the whole cross-section of the beam was solved by a combined electrical lens and analyzer. That device consisted of a series of insulated parallel strips of tantalum being stretched across the tube near its collection end. D.C. potentials were applied to the strips in such a manner as to direct the ions forming the beam to a focal point. A high frequency deflecting voltage was superimposed on the D. C. voltage with its phase such that zero amplitude occurred when bunches of the desired isotope came through the analyzer. For all other ion bunches the high frequency deflecting field was of sufficient strength to deflect the focal beam to one side or the other off of the collector. The analyzer was first tested merely as an electrical lens. There was no difficulty in bringing the beam to a proper focus. Later a degree of separation of uranium isotopes was obtained when the high frequency bunching and deflecting fields were applied.

The results of foregoing operations were in many respects in accordance with theoretical desires but beam currents of one milliampere were of no practical use. To obtain the necessary production a plant capacity of many amperes had to be contemplated and a single unit capacity had to be in the order of amperes.

In order to study the problem of est blishing and maintaining relatively large current in an isotron unit it appeared advantageous to conduct experiments with apparatus other than the isotron itself. A tube 6 inches in diameter and about 24 inches long was constructed together with a pumping system and auxiliary electrical equipment. Both types of sources and many electrode geometries were tried with the tube arrangement. Using a metal vapor source behind a tungsten screen and accelerating the ions to another screen about one cm. away provided a current of 60 ma. from a source of about 20 cm^2 with a total drain of only 180 ma. from the source. The beam down the tube gave evidence of very little spreading; however, it was recognized that additional study was required to assure that the angular divergence of the beam would be held to a few degrees. It was also required that sputtering of the electrodes be studied. With a chloride source the electrodes in the ion path were sputtered away at an appreciable rate as soon as the current density reached the order of 1 ma/cm^2. However, results indicated that uranium ion currents in the order of milliamperes per cm^2 could be obtained and that such currents would be sufficient for isotron operation.

In continuation of experiments on larger operating currents the preceding tube was replaced by a moderate sized complete isotron.

that unit had a tube diameter of 12 inches and was about 8 feet long. These dimensions allowed 160 cm. between the buncher and analyzer and 75 cm. from the analyzer to the collector. The initial experiments with the apparatus were quite successful and served as a basis for calculating the number of stages and units to produce 100 $gram^3/day$, of 95 or more percent pure, U-235. That calculation (reserted as a rough estimate) indicated the requirement of seven stages with 4000 units in the first stage and a total of not more than 4000 units in the six succeeding stages. The cost of such a plant was very roughly estimated to be $10,000,000.

The foregoing operation extended to the late summer of 1942 and then difficulties became apparent. Technical troubles such as leaks and insulation breakdowns developed with both experimental units. Satisfactory separation factors were not obtained. These difficulties, particularly the latter item relating to separation, approached such proportion that doubt of the overall suitability of the isotron method became apparent.

2-5. Review of Isotron Development and Termination of Work - Members of the S-1 Executive Committee met at Princeton on 19 December 1942 to discuss the isotron method of separating isotopes. They were informed that experimental operation had indicated large currents could be obtained in the isotron method but that space charge was apparently preventing suitable separation above a critical current density. Mention was made of the proposal to construct a third experimental isotron and that that unit was to have a 36" tube, with buncher and analyzer areas each 24" x 24". It was hoped that the new unit would

be ready for operation by the end of March, 1943. On the basis of their findings at Princeton the Committee recommended that work on the isotron method be discontinued at an early date as it did not appear that development could be completed within the time available for wartime need.

Acting upon the recommendation of the S-1 Committee the OSRD informed the contractor that the subject work was to be brought to a close on 15 February 1943.

In the two months interval between the review by the S-1 Committee and the actual termination of the isotron work operations were conducted on both experimental units. (Work on the 36" isotron was terminated without its being completed.) Improved results were obtained and there was evidence that a clearer understanding of the causes of the attending problems was being reached.

(Refer to App. B-16, B-17, B-18, B-19 and B-20 for additional details of the isotron development.)

SECTION 3 - THE COUNTER-CURRENT ELECTROMIGRATION METHOD

3-1. General - During the summer of 1941, A. Keith Brewer and S. L. Madorsky developed a reflux electromigration method for the concentration of isotopes. The mechanism presupposed the existence of slight differences in the transport velocities of isotopic ions in solution. The basic principle involved in the process was the establishment of the conditions of counter-current reflux between the isotopic ions migrating in one direction in an electric field and a stream of electrolyte flowing in the opposite direction.

3-2. Theory and Preliminary Experiments - A difference in the transport velocities for isotopic ions had been postulated as early as 1921 by Lindemann. Numerous attempts by Kendall and his co-workers in which ions were electrolysed through long columns of agar failed to detect a separation effect. It was not until the counter-current electromigration method was developed at the National Bureau of Standards that a separation based on ion mobility differences was realized. The process was tested originally with potassium.

Reflux was attained by inducing a counter-flow of electrolyte through a packed tube at such a rate that the net forward transport of K^+ ions was reduced to zero. Under these conditions, the faster-moving ^{39}K ions made headway against the flowing stream and were concentrated in the cathode compartment. At the same time the less mobile ^{41}K ions were carried back towards the anode.

An electrolyte cell used in the concentration of ^{39}K consisted of anode and cathode compartments separated by a tube packed with a uniform-grained porous material (sand). The anode compartment was

connected by means of a siphon to a constant level spillway. Platinum electrodes were placed so as to provide a uniform potential distribution across the packing.

Before starting the run, the cell was filled with electrolyte to the desired level as determined by the height of the spillway. (Either K_2SO_4 or KO_1 solutions were suitable as electrolytes, the effect being independent of the chemical nature of the anion. A concentration of 1 mole of salt to 50 moles of water was found to be convenient although the effect was independent of the concentration between wide limits.)

In operation, K^+ ions migrated towards the cathode compartment while SO_4 ions migrated to the anode. An electrolyte stream-flow was induced in the cathode-to-anode direction by adding a solution of H_2SO_4 to the cathode compartment and KOH to the anode compartment. When the concentrations and rates of addition of both solutions were such that the concentration and pH of the electrolyte throughout the cell remained constant during the run, the flow of electrolyte through the packing was just sufficient to reduce the net-forward transport of potassium to zero. The packing thus behaved as a packed fractionating column operating under total reflux, and ^{39}K concentrated in the cathode compartment.

In addition to the concentrations and nature of the anions, a large number of other variables were tested. These included the electrolysis current, the temperature of the bath in which the cell was immersed, the composition of the packing material and the porosity of the packing. Among the packing materials tested were sand, glass beads, carborundum, asbestos, glass wool, cotton, filter paper, alundum, and rubber battery separators.

The results obtained with potassium were very encouraging. Starting with ordinary potassium in which the abundance ratio is $^{39}K/^{41}K = 14.20$, the ratio was changed to over 20 in less than 200 hours. Likewise, when the cell was run in reverse ^{41}K was concentrated in the anode compartment with equal efficiency.

An analysis of the isotope distribution throughout the length of the packing showed that the entire concentration occurred in about the first centimeter of packing. The most rapid change in composition was found in the first two millimeters adjacent to the cathode compartment.

3-3. Attempted Separation of Uranium Isotopes.- The results obtained with potassium were so encouraging that they suggested the method should prove an efficient means for the concentration of uranium isotopes. During 1942 a large number of experiments were performed using $UO_2(NO_3)_2$, UO_2SO_4, and UO_2Cl_2 as the electrolytes. The results were uniformly unsatisfactory. A change in concentration of only 0.5 percent was the highest obtained by mass spectrometric measurements. Alpha-Count measurements in comparison gave enrichments as high as 2.9 percent. It is believed that the discrepancy between the two methods was due to a radioactive impurity in the uranium.

In applying the ion migration method to the concentration of uranium ions, the National Bureau of Standards had the cooperation of Professor H. S. Harned and his co-workers at Yale University, (App. B-21), of Professor Kraus and his co-workers at Brown University, of Duncan MacInnes and L. G. Longsworth of Rockefeller Institute of Medical Research, and J. W. Westhaver of the U. S. Patent Office.

SECTION 4 - THE COUNTER-CURRENT REFLUX MOLECULAR STILLS METHOD

4-1. General - During February 1941, the research on the separation of isotopes which was being carried on at the Fixed Nitrogen Research Laboratory of the U. S. Department of Agriculture was transferred to the National Bureau of Standards under the direct supervision of Dr. Lyman J. Briggs. For eight months prior to that time, all results obtained relative to this work had been reported exclusively to him.

4-2. Theory and Early Development - Two principal methods of separation in which mercury was used as the test liquid were under investigation, namely: (1) Molecular distillation in counter-current reflux stills, developed by A. Keith Brewer, S. L. Madorsky, and J. W. esthaver; and, (2) Adiabatic distillation in low-pressure stills, developed by A. Keith Brewer and J. W. Westhaver.

The term molecular distillation has been applied to that type of distillation where there is no exchange between the escaping vapor and the evaporating surface. This is accomplished by operating at such low pressures that the mean free path of the escaping molecules is of the order of the distance between the surface of the evaporating liquid and the cooled condensing walls of the still.

In molecular distillation, the relative rate of escape of the various molecular species from a composite liquid surface is determined by: (1) the partial pressure of each component; and, (2) the number of times each component strikes the boundary medium. In the case of the isotopes of the heavy elements in which vapor pressure differences are small, or non-existent, the relative rate of escape of the different isotopes is inversely proportional to the square roots of their atomic

weights.

The isotopes of mercury were first concentrated by Börnsted and Hevesy in 1920 using small single-stage molecular stills. This method was extensively investigated by Harkins and his co-workers at the University of Chicago. While the change in concentration per cell approached the theoretical value, nevertheless, the method was not suitable for obtaining high isotope concentrations because of the very large number of separate distillations that had to be made. The labor involved, the time consumed, and the large quantity of mercury required rendered the method of little practical value. Brewer, Hadorsky and Westhaver developed a new type of molecular still in which any desired number of single-stage stills were so connected together that the resultant separation realized in one operation was equal to the separation per stage raised to the power of the number of stages. All transfer of material between stages took place automatically by gravity feed. As a result, the labor involved in a multi-stage system was reduced to little more than that for a single-stage operation. In addition, the quantity of mercury required, as well as the time of operation, was reduced to such an extent that the method became an efficient process for the separation of isotopes (App. B-22).

The multi-stage molecular still as developed consisted of a series of evaporating surfaces, or pools, set adjacent to each other but at slightly different levels. A cooled roof placed directly above each pool served to condense the vapor. The roofs were so sloped that the condensate ran along the surface and fell into the adjacent cell higher up. Each pool was equipped with a spillway which allowed liquid to run

back in amount equal to the condensed vapor carried forward. Thus, in a series of cells, the vapor from each pool was condensed and allowed to drop into the next higher pool while an equal volume of liquid ran back. As a result, the more volatile material was concentrated in the uppermost cell while the least volatile was collected in the lowest cell. At equilibrium, the difference in concentration between successive cells was the same. The ratio of the isotope abundance on the condensing roof to that of the evaporating surface for any given cell was termed the separation coefficient. The over-all separation factor was the separation coefficient raised to the power of the number of stages in reflux.

A compound to be used economically in a molecular still must be a liquid with a vapor pressure of the order of 1 mm. Hg at the temperature of the evaporating plate and 0.001 mm. Hg, or less, at the temperature of the condensing roof. Since river water is the cheapest source of cooling, the greatest economy could be realized for those compounds whose vapor pressure at room temperature was negligible.

4-3. Separation of Uranium Isotopes - The problem of finding a suitable uranium compound to use in the molecular stills was not simple. A number of double salts of the type $UCl_4 \cdot 2LiCl$ were tested by S. L. Madorsky but they all decomposed at reduced pressures. In addition, a number of solids having appreciable vapor pressures were tested by S. L. Madorsky and T. I. Taylor. These compounds all failed to show an isotopic separation because of the inherent difficulties of mixing solid sources. Tests to find an azeotrope for the chlorides, or fluorides, of uranium resulted in failure. It was not until early in 1943, when Professor

Henry Gilman synthesized $U(OC_2H_5)_5$, that a suitable liquid was available for use in the molecular stills. A number of similar organic uranium compounds were developed at the National Bureau of Standards, and by Professor Gilman, but none proved as efficient s uranium pentaethoxide. The other compounds were either less stable or had too low vapor pressures.

Immediately following the discovery of uranium pentaethoxide, work was started on two general types of molecular stills. The first type developed by Brewer, Madorsky, Westhaver and Taylor was of the high-platage all-gravity feed design. Numerous modifications of the same general design were tested. The most successful design consisted of a long glass tube indented along the bottom at regular intervals to form shallow pools. A water-cooled copper tube, to which copper condensing plates were attached, was then inserted in the glass tube in such a manner that the liquid evaporating from each tube condensed and dropped into the next highest pool. The entire assembly was placed on an incline of about 5° so that the liquid spilled back from pool to pool equal in amount to the quantity evaporating. Stills of this type, 8 feet in length, gave a change in isotope ratio of 8% after operating 25 days.

The second type of molecular still was developed by S. L. Madorsky. It consisted of a number of vertical cells arranged in series in such a manner that counter-current reflux was maintained throughout the entire group. Special magnetic lift pumps were designed to supply the feed between cells. The results obtained from 10 cells operating in series gave a separation coefficient of 0.1 percent per theoretical plate. This was to be compared with a theoretical value of 0.29 per

plate.

The results on molecular distillation show that molecular stills can be connected in series and made to operate in unison under counter-current reflux, and the isotopes of mercury and of uranium have been concentrated in such stills. Although the theoretical separation efficiency was not attained for the isotopes of uranium, the method presents certain advantages from the standpoint of power consumption since temperatures not to exceed 150° C are required for the evaporating surfaces and river water can be used for cooling the condensing surfaces.

The National Bureau of Standards had the cooperation of Professor Henry Gilman and his co-workers at Iowa State College of Ames, Iowa, in developing the $U(OC_2H_5)_5$ and other related compounds used in this research; and of Mr. J. W. Westhaver of the U. S. Patent Office in working out much of the theory involved.

SECTION 5 - THE FRACTIONAL SUBLIMATION METHOD (OF URANIUM ISOTOPE SEPARATION)

5-1. General - During the early part of 1945 a method of separating uranium isotopes by alternately vaporising and condensing UF_6 was proposed at Columbia University. The process appeared novel and, on the basis of the then meager data from a limited number of experimental runs, gave some promise of success.

Briefly the method proposed consisted of moving process gas in a tube through successive hot and cold zones so that vapors were continually formed and partially condensed. Under such arrangement it was indicated that the isotopes would move toward the opposite ends of the tube. Under the experimental setup, 5/8" copper tube, in the form of a 22 turn, 6" diameter, helix, was rotated slowly (1.05 r.p.m.) on a horizontal axis through stationary hot and cold zones. The test unit, exclusive of drive mechanism, refrigeration, etc., occupied a floor space of approximately 6 square feet. A more complete description of the equipment and method of operation is provided by the contractor's report (Appp. B-23).

The potential advantages offered by the system seemed to be: (1) low capital cost of plant due to relatively simple equipment and floor space requirements; and, (2) low operating and maintenance personnel requirements.

Analytical ta and early results obtained for the subject method of isotopic concentration were somewhat questionable and emphasized the need for further tests to substantiate the enhancement reported by the contractor. That condition led to the recommendation

that a program of study be authorized to: (1) establish conclusively the separation reported; (2) determine the effects and importance of various process variables; (3) theoretically study the principles underlying the functioning of the process; and, (4) conduct a chemical engineering review of the method to determine the type of equipment, process, and controls to be used in a production plant.

Subsequent investigation revealed that little or no separation of uranium isotopes could be depended upon for the sublimation-condensation process. This outcome had the full concurrence of all concerned and the process was dropped as being entirely unsuccessful.

SECTION 6 - THE ETHER-WATER METHOD

6-1. General. Preliminary experiments by Drs. F. B. Brown and B. Weinstock, at Columbia University, in August, 1942, indicated that a fractionation of uranium isotopes took place when an aqueous solution of uranyl nitrate was extracted with ether. The process is described briefly by the following outline of one of the experiments conducted at Columbia. An ether solution containing 10 kilograms of uranyl nitrate was shaken with sufficient water (20% by volume) until distribution equilibrium of the salt between the two layers was attained. The water layer was then removed and the concentration restored to its original value by evaporation of the ether. A small amount of water was added to the solution to effect additional compensation. The process of extraction was then repeated using 20% by volume of water which was equilibrated with the ether layer and subsequently removed. Forty extractions of this type were usually employed, which left about one gram of uranyl nitrate in the ether layer. Thus the reduction of the uranyl nitrate was from 10,000 to 1 and, as was reported, yielded a fractionation factor of 1.00063 (App. B-24).

On the basis of the above an estimate was made of the size, power requirements and cost of a plant to double the concentration of U235. That estimate indicated that such a plant could be constructed without a great drain on then strategic materials and that it would not be unduly costly.

Subsequent to the above activity at Columbia, work on this project was carried on simultaneously at Yale University, at the University of

Pennsylvania, and at the duPont Company laboratories. At Yale University, Professor Harned and his co-workers repeated the original experiments of the Columbia group, and also made many new extractions at different temperatures (App. B-21). The work of Dr. M. Kilpatrick, at the University of Pennsylvania, provided a great deal of information with respect to the analytical procedures for uranyl salt solutions, and, also, the settling times for mixtures of ether and water solutions of the salt (App. B-25). The duPont Company work was directed toward the determination of the necessary large scale apparatus (App. B-26).

The foregoing study at Yale University showed that the fractionation factor for the ether-water method was not so large as had been expected. In fact, Report A-713 (App. B-21) states in part, regarding samples analyzed, that they "showed no increase in U^{235} whatsoever, so that the original favorable prognostication was proved to be entirely erroneous." The statement contained in Report A-713 was considered to have been proven by the results of ten well conducted experiments.

In view of the foregoing the subject work was discontinued and the project was abandoned.

SECTION 7 - THE IONIC CENTRIFUGE METHOD

7-1. General. Early in 1942 Dr. J. Slepian joined the group at Berkeley to engage in work on electromagnetic methods of separating uranium isotopes. Slepian had conceived a particular method known as the ionic centrifuge, and his activity at the Radiation Laboratory, until the end of the year, was confined to the development of this conception. After 31 December 1942, further investigation of the process was transferred to the Westinghouse Research Laboratories, East Pittsburgh, Pennsylvania, where it was continued until the termination of the Manhattan District control. Appendix A-19 of this history provides an account of the basic theory, the difficulties encountered, and the results obtained for the ionic centrifuge during 1942. In general that account indicates that some separation of the desired material was obtained; results were not consistent however, and necessary developments were not completed in time to offer promise of fulfilling the war time need. Limited quantities of uranium were made available to Dr. Slepian for his continued investigation, but further activity at the Westinghouse Research Laboratories, before the termination of Manhattan District control failed to change appreciably the status of this development.

SECTION 8 - THE PHOTOCHEMICAL METHOD

8-1. General. Early in 1943 a program was initiated to determine the feasibility of separating uranium isotopes by a photochemical method. Initial work was undertaken by the Chemical Division of the Columbia Group under the direction of Professor H. C. Urey. The probability of the success of such a method was dependent upon whether or not a difference could be determined in the absorption spectra of the isotopes and then whether or not that difference could be exploited to obtain separation. It was believed that a suitable mixture might be illuminated by light which would be predominantly absorbed by only one of the isotopes, thereby bringing it into an excited state. If a reaction could be found which would take place with the excited atoms only and that reaction product could be removed from the mixture, it would, in an ideal case, contain the desired isotopes exclusively.

It was fully recognized that if such a separation method was to be successful the following conditions had to be fulfilled:

a. There must be a difference in the absorption spectrum of compounds which were identical except for their content of U235 or U238. That difference might be either in the position or the intensity of an absorption line.

b. It must be possible, to the greatest practicable extent, to illuminate the compound with light containing the absorption frequency for U235 but [illegible] for U238. In that manner the molecules containing U235 would be more readily excited than those containing U238.

c. There must be a chemical reaction which would affect the

excited molecules and remove them. For this condition it was important that the excitation energy not be exchanged from a U235 molecule to a U238 molecule before the reaction could occur.

8-2. Investigation and Research. The foregoing requirements could not be investigated with equal ease, and, as study of the absorption spectrum would present information of suitable substances upon which further work should be done, it was advisable to concentrate in the beginning upon that subject.

Most of the spectrographic work was carried out at Columbia University, but some of that research was conducted at Johns Hopkins University, and X-ray investigations of the crystal structure were conducted at Cornell University. The exploratory spectrographic work at Columbia extended from February to December of 1943. Toward the end of 1943, in conducting a study of the spectrographic behavior of a uranium compound in a magnetic field, it became advisable to work with higher dispersion than was available at Columbia. A series of experiments was therefore made in December, 1943, at Johns Hopkins University, where such facilities were available. All spectroscopic work, with one exception (UF_6), up to June, 1944, was carried out with compounds containing ordinary uranium. In 1944, samples containing chiefly U235 became available and from 1 July of that year to 31 January 1945 spectrographic studies were made at Johns Hopkins with selected uranyl compounds prepared from uranium samples containing about 80% U235.

Detailed investigations on the structure of the spectra were carried out at 20°K, the temperature of liquid hydrogen. In this connection it

was fully appreciated that any large scale process which might be feasible for photochemical separation of uranium isotopes would in all probability have to be carri l out at a higher temperature.

The early research of the project was greatly aided by use of a collection of various uranyl compounds in cystalline form which had been prepared at Cornell University more than twenty years previously. However, it was necessary that crystals used in the latter part of the work be grown on the spot. Facilities for this were set up at Columbia University prior to termination of the project work.

8-3. Investigation Results and Termination of Work. The differences in the spectra of uranium salts where U238 was replaced by U235 were completely solved for two compounds, as far as wave length shifts are concerned. The results on intensity changes, however, required further study and were not conclusive. Because of incomplete knowledge of the fundamental structure of the spectra of these compounds a definite answer to the question of how other uranium compounds would behave under isotopic substitutions was not obtained.

Even though definite answers to all questions were lacking, the results of the project investigations were such as to permit a statement on the feasibility of a photochemical separation of uranium isotopes. If the two compounds investigated in detail are typical of other uranium compounds, the changes in the absorption spectra of uranium compounds when U238 is replaced by U235 are too small to make a photochemical process utilizing these differences practicable.

The preceding is a short summary from a final report made of the photochemical investigation. For details of the problems which existed,

the investigations made and the results attained on this project the reader is referred to that report (App. B-27).

8-4. Personnel. Scientific personnel prominent in the project investigations were as follows:

H. C. Urey directed the project throughout its existance.

The administrative supervision was carried out by H. C. Crist during the early stage, and by M. Kilpatrick during the later part of the work.

Direction of certain of the exploratory work on the photochemical process was assumed by C. E. Herrick, Jr.

A.B.F. Duncan and S. Freed directed the exploratory spectroscopic work at Columbia University.

The spectroscopic work at Johns Hopkins University was under the direction of G. H. Dieke.

Study of molecule structures as derived from the spectra they produced was conducted by Mrs. M. G. Mayer.

Work in obtaining the crystal structure of a uranium compound was done by J. L. Hoard at Cornell University. R. C. Gibbs cooperated in making the Cornell collection of uranyl compounds in crystalline form available for project study.

Columbia facilities for the preparation of crystals were under the direction of L. Gilbertson, S. Freed and H. H. Taylor.

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MANHATTAN DISTRICT HISTORY

BOOK I - GENERAL

VOLUME 4 - AUXILIARY ACTIVITIES

CHAPTER 14 - INVESTIGATIONS OF MISCELLANEOUS PROCESSES

OF SEPARATION OF URANIUM ISOTOPES

APPENDIX A - DOCUMENTS

No.	Description
1.	Report - By Dr. Z. G. Deutsch (report is unsigned but author is identified by correspondence not included among following documents) - Refers to uranium hexafluoride as feed material for centrifuge process plant.
2.	Report - By Dr. Z. G. Deutsch (identification of author as shown for item 1) - Describes proposed production plant for separation of uranium isotopes by centrifuge method.
3.	Design Proposal - Outline of gas centrifuge plant, to operate between concentration levels of 36% and 90%, for production of 1 Kg/day of material - dated 15 March 1943.
4.	Letter - From Dr. W. V. Murphree to members of S-1 Committee, 6 December 1943 - Reviews gas centrifuge pilot plant progress.
5.	Letter - From Dr. H. C. Urey to General L. R. Groves, 7 December 1943 - Provides comparative figures on gas centrifuge and gas diffusion projects.
6.	Letter - From Dr. H. C. Urey to Dr. J. B. Conant, 11 December 1943 - Requests that investigation be made of centrifuge method.
7.	Letter - From General L. R. Groves to Dr. J. B. Conant, 18 December 1943 - Requests view, from scientific standpoint, on feasibility of centrifuge project.
8.	Letter - From Dr. R. C. Tolman to General L. R. Groves, 20 December 1943 - Refers to inspection of Bayway Plant and discusses merit of centrifugal method.
9.	Letter - From Dr. W. V. Murphree to Dr. J. B. Conant, 22 December 1943 - Rough time schedule for construction and operation of centrifuge plants for production.

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10. Letter - From Dr. R. V. Murphree to Dr. J. B. Conant, 22 December 1943 - Outline of experimental program based upon favorable consideration of continuation of centrifuge project development.

11. Letter - From Dr. J. B. Conant to General L. R. Groves, 28 December 1943 - Expresses unfavorable view toward continuation of centrifuge program.

12. Letter - From Dr. R. V. Murphree to General L. R. Groves, 31 December 1943 - Suggests appointment of committee to provide recommendation regarding continuance of centrifuge program.

13. Letter - From Dr. E. O. Lawrence to General L. R. Groves, 1 January 1944 - Recommends termination of the centrifuge program.

14. Letter - From Dr. H. C. Urey to Dr. J. B. Conant, 3 January 1944 - Indicates favorable opinion of centrifuge method.

15. Letter - From Dr. H. C. Urey to General L. R. Groves, 3 January 1944 - Recommends appointment of a committee to study the centrifuge method.

16. Letter - From Dr. L. J. Briggs to General L. R. Groves, 3 January 1944 - Recommends continuation of the centrifuge program be dependent upon the probable duration of the war.

17. Letter - From Dr. A. H. Compton to Dr. J. B. Conant, 12 January 1944 - Express opinion that centrifuge production plant should not be undertaken.

18. Letter - From General L. R. Groves to Dr. J. B. Conant, 19 January 1944 - Indicates no further extension of the centrifuge project is justified

19. Report NRRB Vol. IV Part III Chapter I - By J. Slepian - The Ionic Centrifuge, Work on an Electromagnetic Isotopic Separator Carried on at the University of California Radiation Laboratory, 1 January 1942 to 31 December 1942.

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PART NO. 1

As Presented to Executive Committee Meeting October 23, 1942

REACTIVITY

All development work, toward a design of plant for the separation of our isotopes has visualized working with a single material -- uranium hexafluoride. A gaseous material is desirable in order to hold the process inventory to a minimum. A low process inventory is essential to early production of a material present in such a minute concentration. Uranium has only two known compounds with vapor pressure high enough to permit vapor phase handling. One of these was discovered quite recently, and its properties, though not as yet well delineated, are probably less favorable than those of the hexafluoride. The hexafluoride has the advantage of being gaseous at low operating pressures and moderate temperatures. The principal objection to it is its extreme chemical activity.

It is an especially active fluorinator. It probably reacts instantly with most clean metals, forming the tetra or penta fluoride of uranium and a fluoride of the metal in question. Some fluorides (like oxides) form durably tough protective films on the metals, thereby giving us suitable materials of construction, which will be discussed beyond. Some of the other metal fluorides are not durable, or are gaseous; consequently, such metals are unsuitable as structural materials.

Hex likewise reacts with practically all non-metals to form fluorides. Hydrogen containing materials are particularly reactive, and the fluorine in hex will replace other halogens in many compounds.

A unique characteristic of hex is its extreme readiness to react with water, forming uranium oxyfluoride and HF. This mixture is of course reactive in itself, and consequently the corrosive properties of hex often start a secondary or tertiary reaction if any water vapor or hydrogen containing compound is present. Hex can not be allowed to come in contact with ordinary air because it reacts with substantially all of the water vapor contained therein. A cubic foot of ordinary atmospheric air contains enough water vapor to destroy about 7 grams of hex. Thus, if only one cubic foot of air per hour leaked into the piping at the enriched end of a system, as much as 10% of the product could be lost.

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VAPOR PRESSURE-TEMPERATURE RELATIONS

On the screen is shown a chart giving the approximate relationship of vapor pressure to temperature. It will be noticed that hex is a solid crystalline material at most room temperature conditions. The crystals are generally quite massive, are white, and in a fairly finely divided condition have a bulk density of about 2.2. At low pressures and ordinary room temperatures, the crystals sublime directly to the gaseous state. At 20°C. this takes place at about 8 cm. of mercury absolute pressure. In the proposed centrifuge plants, it is expected to handle the material at less than 3 cm. absolute in all of the piping. In the centrifuge bowls, the centrifugal action increases the pressure toward the periphery, the pressure ratio being about 60:1. Therefore, to maintain the material in the gaseous condition, the centrifuge bowls will have to be kept at an elevated temperature. The diagram indicates that this will be about 75°C., and at that level will provide some factor of safety to prevent liquefaction within the bowl.

MANUFACTURE

The Harshaw Chemical Company of Cleveland, Ohio, has manufactured something over 350 pounds of hex by the method which is described beyond. Their operation was batch-wise, and now after having established the "Know-How", it is believed to have a capacity of somewhere between 10 and 25 pounds of hex per day. The duPont Company, in their Jackson Laboratories at Deepwater, N. J., have manufactured about 650 pounds by the same process in somewhat bigger apparatus which has a capacity of 25 pounds per day.

The process used by both firms was developed by Dr. Fowler of Johns Hopkins University. The uranium oxide of commerce, U_3O_8, a fairly pure product, is reduced in stainless steel equipment, using hydrogen gas from cylinders, maintaining a temperature of 400-600°C. by electric heating. The UO_2 resulting from this operation is placed in trays in a nickel or copper reactor and is converted into UF_4 by direct contact with anhydrous hydrogen fluoride, maintaining the temperature at about 550°C. Next, the UF_4 in the same or similar vessels is converted into hex by contact with elemental fluorine, the product passing off as a gas from the reaction at about 250-400°C. and being collected in cold traps. The fluorine is manufactured by the electrolysis of KF-HF mixed electrolyte. The final product, being gaseous, is fairly pure. The principal impurities are HF and molybdenum fluoride. These latter two can be removed by a sort of fractionation process for which both Harshaw and duPont are now set up or a small scale.

For the production of about 1,400 pounds per day, such as is needed in a large plant, it is proposed to design a continuous apparatus based on the knowledge gained in the batch apparatus.

MATERIALS OF CONSTRUCTION

Some metals are quite resistant to hex. According to our present knowledge, these are nickel, aluminum and copper. The earliest corrosion tests indicated that the hex was very corrosive, but it has gradually become evident that much of this was due to the presence of HF, originally in the hex or else formed by the corrosion or decomposition in the presence of some non-corrosion resistant material, such as grease or dirt on the metal samples or parts of the test equipment, such as lead gaskets. Nickel is probably the most corrosion resistant material, since tests indicate that it is resistant to hex at higher temperatures than the other metals. Copper is a close second. Monel metal, low tin bronzes and beryllium copper have been tested, and all appear to be satisfactory when clean samples are subjected to pure hex. Most aluminum alloys, such as the Duruls, are likewise good, but apparently begin to corrode at somewhat lower temperatures than the above two metals. Even so, the rate of corrosion considered from a structural angle is thought to be very low, although our data are as yet incomplete. A few specimens of magnesium alloys have been tested and have been found to be resistant.

Metals about which we are less sure are mild steel, the stainless alloys, and some nickel plated strong steel. Mild steel apparently resists corrosion reasonably well if meticulously cleaned before contact. However, the results are somewhat erratic. In the centrifuge plant, no mild steel is expected to be subjected directly to gas contact.

The stainless alloys at first appeared to suffer from inter-crystalline attack. The data on these alloys are meager, our latest samples showing up considerably better.

The present plan is to make centrifuge bowls out of an aluminum alloy known as 14 ST, which contains about 4.4% copper, about .8% silicon, .8% manganese, .4% magnesium, and the balance aluminium. The strength over density ratio of this alloy is higher than all but the very strongest of alloy steels. If the strongest alloy steels could be used in bowls, a higher speed, and consequently a considerably lower plant investment, might be possible. Thick nickel plating on such strong steel might permit its use. Laboratory

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results have not been entirely encouraging, but a single experimental centrifuge bowl is being made. Some new Mg. alloys might be competitive with 14 ST aluminum.

Certain metals have been found definitely to corrode too rapidly to be of use. The high silicon alloys are inferior. Lead is not resistant. A high strength steel containing a small amount of molybdenum has been found to be considerably inferior to mild steel. Brass (high zinc alloy) has also been found to be inferior.

Among the non-metals, the only things which are found to be thoroughly resistant are fluorides or completely fluorinated compounds. A number of fluorocarbons have been prepared which seem to be entirely resistant, but practically none of these has desirable structural properties.

Present corrosion tests, besides being aimed at disclosing more structural materials, are being conducted to determine with precision the rate of destruction of hex when in contact with the present "best" materials, i.e., Duralumin, copper, nickel, etc.

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PART NO. 7

As Presented to Executive Committee Meeting October 23, 1942

FLOW DIAGRAM

In a large plant for the separation of the isotopes of uranium, a flow diagram in the ordinary industrial plant sense is almost meaningless. It is expected that the uranium hexafluoride will be made at the site, and hence the raw materials coming into the plant will be commercial uranium oxide and hydrogen fluoride.

The product and the by-product are both uranium hexafluorides, there being four pounds of product and 1400 pounds of by-product or waste. This latter will probably be decomposed to facilitate handling, and re-sold on the usual uranium-salt market.

MAIN UNITS - SLIDE NO. 2

The large plant at present visualized is intended to have a capacity of 1 Kg. of 90% U_{235} metal per 24 hour day. Considerations have narrowed down to the use of a single size of centrifuge having a cylindrical bowl about 8" in diameter and about 11' long. This relationship has been adopted after an amount of study compatible with the urgency to freeze to a single mechanical design problem. It is anticipated that bowls will operate at a peripheral velocity of 2.7×10^4 cm/sec., or about 10 miles/min. As explained by Mr. Scharmann, this velocity causes a centrifugal stress which comes fairly close to the yield point of the strongest duralumin. Until the possibility of using especially strong alloy steels is better understood, we must therefore content ourselves with the above mentioned peripheral velocity. In general, the use of very long bowls reduces the number of machines which must be driven, and the use of very small diameter bowls reduces the inventory of process gas. Both of these tendencies are desirable.

However, smaller diameter bowls to operate at the same peripheral velocity require higher rotational speeds which introduce mechanical difficulties and greatly increase the driving power.

Bowls considerably larger in diameter than the 8" would markedly decrease the power consumption of a plant,

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but they markedly increase the inventory in process. We believe that the present diameter of about 8" is near a fairly flat optimum, although all of the complex factors involved in the problem have not been evaluated. The length of 132" is the maximum at present obtainable with modern forging and machining facilities for such structures.

The plant using this size centrifuge will require 25 enriching stages involving about 14,000 individual machines. Two more stages involving about 3,000 additional machines are needed to strip the waste material down to about two-thirds of its original content of the valuable isotope. On the slide are tabulated the relevant factors of such a cascade; the last column to the right shows the concentration expected for the valuable isotope in each stage. The inventory of material in process is also shown for each stage. On this basis, the estimated minimum time for the plant to reach equilibrium, assuming a methodical starting-up procedure throughout, would be about 85 days.

PLANT ARRANGEMENT - SLIDE NO. 3

A practical plant for the accommodation of a cascade of 17,000 units as just described calls for a site with the usual topographical requirements, except that transportation of raw material and finished product is substantially absent as a factor. A large source of cheap electric power, and a fairly large source of cooling water are the principal requirements. The arrangement of the several principal buildings and necessary auxiliary structures thus require only that the shops and services be reasonably well centralized, and that the buildings for the centrifuges be fairly close to the same level.

The centrifuges will occupy a building space totaling about 15 acres. This is based on a 5' spacing from machine to machine in each direction with rows of 5 lines, each serviced by a simple traveling crane. It is possible that in the pilot plant a spacing of less than 5' might be found to be practical, if not in both directions then possibly in one of the two directions. It must be remembered that there are a total of about 40 pipes, tubes or wires connecting to each unit, and that the manifolding of these connections require numerous specifications to be met.

The machines will best be arranged in separate groups of buildings. On Slide No. 4 is shown a proposal for such an arrangement, which grows out of two principal requirements:

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A. The construction of such a large plant requires a methodical schedule to permit progressive manufacture and erection of the large number of units, and a progressive readying for operation. Actual operation of first stages can precede erection of higher stages.

B. A total of 15 acres of nearly flat land would be needed in order to put all machines under a single roof. If grouped with some reference to staging, it will be possible to permit a variation in level from group to group provided it is held within due bounds.

Slide No. 4 shows stripping, all to be done in one building, and up to the fifth stage the machines in each building all operate at the same concentration level. From then on up, the machines corresponding to two or more stages are housed in a single building.

Within each building, machines will be divided into blocks. It is necessary to start blocks of the machines, as for example, 25, 50 or 100 at a time, so as to minimize the electrical equipment needed for their acceleration and to reduce the number of switches. Likewise, the mechanical valving of the process gas and auxiliary pipelines is such that it will be much more expedient to valve off blocks of 25, 50 or 100 rather than individual machines.

The starting or accelerating of these main units requires the use of rather special accelerating motor generating sets. In order to avoid operating the driving motors for each centrifuge at light and inefficient load when at full speed, the acceleration will necessarily have to take a long time. Consequently, a fairly large number of accelerating motor generator sets would be needed if the groups of machines starting together were too small.

On Slide No. 5 is a rough indication of the location of the principal auxiliaries to these main units. The electrical auxiliaries will probably be housed in a single building so as to afford adequate protection and minimize one type of attendance. The mechanical auxiliaries on the other hand will probably be divided, some of them being immediately adjacent to the main units, and others being centralized among the centrifuge buildings. The main plant services, that is the power plant for the generation of emergency electric power, auxiliary and heating steam, together with the water pumping plant and the shops, stores, warehouse facilities, change rooms and the like, will be located to suit the topography of the site selected, with due regard to centralization and protection.

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It is visualized that the operation of the centrifuges would be supervised by men stationed in each of the buildings. It is intended that each centrifuge casing be strong enough to absorb the entire energy if a defective bowl should explode. Hence no barriers between machines are visualized. The operating stations will be principally at control boards and among the auxiliaries which require routine attendance. Slide No. 6 indicates cross sections through one of the larger single-stage buildings, together with a plan of one corner of such a building. Attendance aisles for routine inspection of the machines are visualized as running above and perhaps below the machines. In the fairly large space below the machines will be piping aisles and some of the more scattered auxiliaries. Around the periphery of the building will be the instrument panels and the principal control stations. The operations to be conducted by the attending personnel are best visualized by a discussion of the auxiliaries to the main units which are listed on Slide No. 7.

AUXILIARIES

A. Lubricating Oil

A low viscosity lubricating oil will have to be fed to about 10 points on each machine under fairly high pressure. A continuous and unfailing supply of this oil is of tremendous importance, and one of the principal design features of this auxiliary is to provide such continuity. Furthermore, substantially all of the power consumed in the operation exhibits itself as temperature rise in this oil. Therefore, rather extensive cooling installations will be required, and it is visualized that these will be separate for relatively small banks of centrifuges. The separate groups of machines for the lubricating oil are important because drainage of spent oil will probably best be through a gravity system. In the machine, the oil forms a vacuum seal at several points, and it is therefore necessary that a thoroughly degassed oil be charged into each system. It may also be necessary (although at present it is not contemplated) to deaerate some of the oil continuously. In any case, deaerating equipment for the initial charge and make-up charges will have to be provided as part of the lubricating oil system. Also a continuous purification system for removing foreign bodies, products of gradual decomposition, etc., will have to be provided, similar to those in turbine power plants.

B. Casing Temperature Control

As indicated in the first paper presented today, hex will have to be kept above 75°C. at the periphery of each centrifuge bowl to prevent its liquefaction. If it should liquefy within the bowl, flow would soon be interrupted, and enough may accumulate to burst the bowl. Aside from the danger of liquefaction, a constant and precisely controlled temperature is of extreme importance so as to prevent any loss of separative work through thermal turbulence. The casing of each machine will be equipped with a coil through which water will be circulated in order to supply the heat of radiation and maintain the uniform conditions described above. For each building, it is contemplated that water would be circulated at about 2500 gals./min. to and from a thermostatically controlled reservoir. The reservoir would have the triple function of providing a large body of temperature-constant water, regulating the make-up of heat and the make-up of fresh water. Purification of the water may be either periodic or continuous.

C. Hydrogen

The space between the rotating bowl and the stationary casing must be operated at a fairly high vacuum. An extreme vacuum is not feasible because of the necessity of maintaining temperature control through the conduction of heat from the casing to the bowl. To facilitate this heat transfer and yet minimize the windage, it is planned to feed each casing with hydrogen at about 1 cm. Hg. absolute pressure. It will be necessary to keep a small flow of hydrogen through each bowl so as to sweep out lubricating oil vapors. It is anticipated that the hydrogen will be generated in a single central building, probably by electrolysis of water. The hydrogen gas will be dried at atmospheric pressure and fed to each machine, probably near the center of the casing, in a continuous stream controlled by means of an acoustic tube or orifice. Outlets for each casing will be at the two ends of the machine connecting to a common manifold and separate vacuum maintaining compressor system. Blocks of one or two hundred machines will probably be handled on separate casing vacuum systems with cross-overs for emergency and stand-by service.

D. Gas Pump and Motor Cooling

As Mr. Scharmann has explained, a small gas compressor or centrifugal pump is located in each end of each centrifuge. The electric drive motor is located at one end. A small part of the total heat which must be dissipated from these plants must be taken from these three units. A built-in coil will be on each unit, arranged to take a small flow

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of cooling water. Temperature control is not of prime importance here, and so the three coils will be operated in series to simplify piping. The water will come directly from the source of cold water for the entire plant, and the outlet water will go directly to the sewer.

E. Nitrogen

As explained in Mr. Scharwann's discussion of the machine, nitrogen gas is fed to two points at the top and two points at the bottom of each machine. In order to avoid all possibility of reaction with the process gas, this nitrogen will have to be reasonably pure and perfectly dry. It is at present planned to prepare nitrogen by fractional distillation of liquid air. Further study may show that a cheaper source of inert gas might be as adequate. In any case, the final buffer gas will probably have to be stored so as to provide the advantages of an accumulator in maintaining the especially precise pressure control which will be required. A step-wise pressure reduction to a precisely controlled feed manifold pressure is contemplated. The evacuation of the nitrogen takes place through the process gas recovery equipment described in the next section.

F. Separation of N_2 From Leak-Off Gas

Two small streams of process gas mixed with nitrogen will leak off from the top, and two more from the bottom of each machine. These streams must be handled separately for each stage in order to avoid any loss of separative work. In the lower stages and in the strippers, this involves a fairly large number of recovery units for each stage, perhaps one for each 50 machines. In the richest stages, smaller recovery units are contemplated. The recovery method at present appearing most feasible is that of chilling of the gas stream with continuous trapping out of the hex snow formed, and its continuous re-evaporation into the system. Other separation methods have been studied, a few discarded as impractical; some are still being analyzed as alternate possibilities. Among these are mentioned solvent extraction from nitrogen with a carbofluoride, and the use of a carbofluoride buffer gas instead of nitrogen, from the mixture of which fractional distillation separations might be made.

In all of them, the waste nitrogen or buffer gas or solvent must be stripped to such an extremely low hex concentration as to make any of the separations difficult for this reason alone.

The refrigeration separation seems to offer the most promise, because a safely low temperature for thorough

stripping of N_2 is moderately easy of attainment through the use of ethylene refrigerant. The device at present being built for this recovery in the pilot plant is a heat interchanger whose inner sides are continuously scraped. The leak-off gas being cooled in its passage deposits solid hex on the chilled walls. The hex snow dislodged by the scrapers is collected in the bottom of the device where a powder seal removes it continuously to a heated chamber below. From the heated chamber, the vaporized hex returns to the system. The chilled nitrogen from the top of the cold trap goes directly to the vacuum pumps which provide the pressure control on the entire centrifuge-shaft seal system.

G. Pressure Change

As mentioned previously, in order to obtain a fairly low plant equilibrium time, it is contemplated that machines in the richer stages will be operated with .8 cm. absolute pressure at the axis rather than the 1.6 cm. for the lower stages and stripper. If all of the machines from the 12th stage up to the richest stage will be operated at reduced pressure, the content of each bowl up in the richer stages will be about half that in each bowl below this point. This will require merely the introduction of a pressure control throttling device for the enriched stream, and a pressure boosting pump for the reflux stream.

H. Raw Material Preparation

It is anticipated that the feed stock of uranium hexafluoride would be prepared at the plant from the commercial uranium oxide and commercial hydrogen fluoride. Approximately 1400#/day of feed stock will be required in a 1 kg. plant. The steps of reduction, fluorination and perfluorination can no doubt be carried out in continuous rotary apparatus similar to that at present used on many analogous roasting operations. Since all of the feed stock will be used in the first stage building, it is contemplated that this operation will be immediately adjacent to the machines forming the first stage.

I. Waste Handling

Substantially as much hex is discharged continuously from the stripper stage as is fed to the first stage. This material could be handled and shipped away in evacuated and re-pressured cylinders, and as such does not represent any special problem, there being about ten 50#

drums to fill and handle per eight-hour shift. However, it is likely that it would be better to convert the material to a more easily handled product for sale to present consumers of uranium salts.

The desired end-product from a 1 kg./day plant weighs less than 4#, and can therefore be shipped each day in a small nitrogen repressured cylinder without the need of any special auxiliary equipment.

J. Frequency Conversion

Somewhat in excess of 40,000 KVA of electric power will be absorbed in operating the 17,000 main machines. This power should undoubtedly be purchased as high tension 60 cycle alternating current. It is anticipated that it will be stepped down to the 6,600 volt range. As such, it would be fed to the motor end of the high frequency generators. At the generator end, 480 cycle current would be generated at 4,160 volts for distribution to the centrifuge buildings. 1,000 KVA transformers to step down to the 440 volt motors would be provided for perhaps each 400 centrifuges. 4,000 KW MG sets appear to be the size which we are likely to use. Therefore, an important auxiliary will be one building housing some 15 5,000 HP MG sets which are called the "running" units. The "starting" units which consist of a pair of variable speed MG sets, with a rotatrol for constant torque acceleration, will more likely be closer to the centrifuges.

CAPITAL COST - SLIDE NO. 8

It should be apparent by now that many important decisions regarding a large plant such as I have been describing are yet to be made. The general nature of the plant has been fairly well visualized, and preliminary designs made. Consequently, our estimate of capital cost is necessarily a very approximate one, intended principally to show the scope of the project. On the slide are three sets of figures. The first represents the totals of materials which will be required in their raw state for the construction. For example, the 10,000 tons of aluminum represents the weight of the rough extruded tubes and unmachined forgings which will have to be shipped to the fabricator of the centrifuges.

The second set of figures gives the roughly estimated dollar cost of the project up to the point when all equipment shown on the design drawings will have been set up in place.

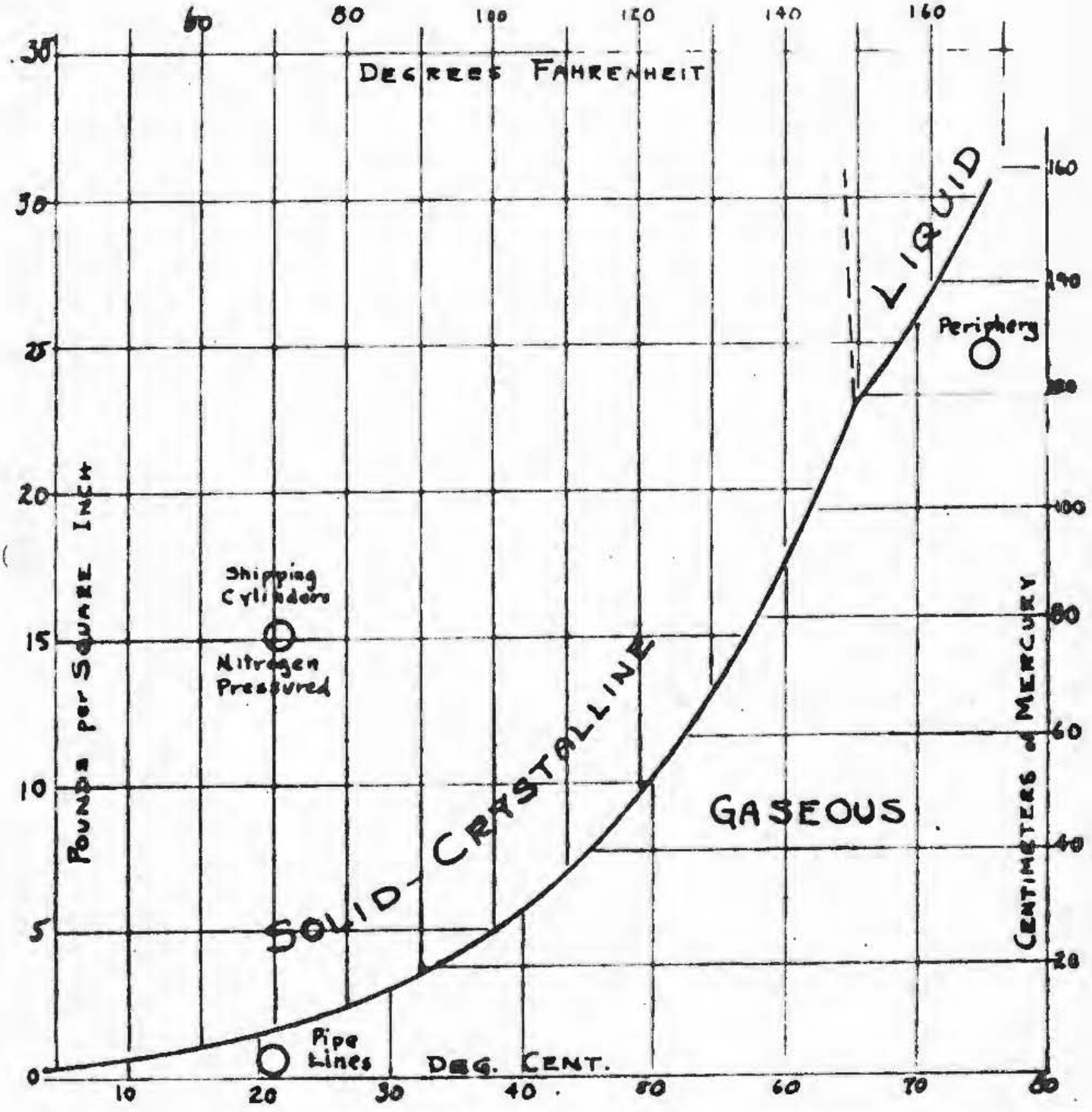
The third set of figures represents an expected expenditure on extra operating personnel during the final stages of construction; on the expendable materials, supplies and power needed to get the plant into operation, together with extraordinary mechanical and electrical changes which may be required.

OPERATING COST

Slide No. 9 is perhaps self-explanatory. It shows the number of people which will probably be required to run such an operation as we visualize, at least during its first year. It is believed that the consumptions of operating labor, power, etc. have all been estimated conservatively in the light of our incomplete analysis.

In conclusion, I wish to point out that although the foregoing indicates an extensive operation, requiring meticulous care in its construction and control, and utilizing expensive materials of construction, it is not without some precedent in modern industrial practice. It is no doubt true that there is hardly any precedent for a production cost of the order of $16,000/lb. Perhaps 35 miles of spinning bowl may appear inordinate for any purpose. But in many plants, equipment of such astronomical magnitude is to be found. Consider, for example, a typical rayon yarn factory. A moderate sized plant will have perhaps 400 so-called spinning machines. Each of these machines has built into it about 120 high-speed centrifuges making perhaps 50,000 centrifuges in a common rayon plant. It is true that these are considerably smaller than the centrifuges we contemplate, but it is also true that for each centrifuge there is a viscose solution pump requiring phenomenal precision in its parts and performance. There is also, for each of the 50,000 centrifuges, a platinum spinneret with as many as 30 minute holes in it. Other quite special and expensive parts are commonplace. The entire spinning machine is constructed of lead and glass and special porcelain or platinum parts, and yet it produces a material between cotton and wool in value. The rayon industry in its control forms a further good analogy to the proposed centrifuge plant. Meticulous care in the preparation of the initial solutions; in the avoidance of contamination by any number of fatal impurities (especially tiny air bubbles) is demanded. Very precise control of temperatures and concentrations are needed throughout. The rayon industry has found means of overcoming all such obstacles, and is able today to control its plants with ordinary labor such as can be recruited in any country.

SLIDE NO 1.



TEMPERATURE~PRESSURE RELATIONSHIP

TABLE NO. 1

NUMBER OF MACHINES AND CONCENTRATION GRADIENT IN CASCADE

ENRICHING SECTION

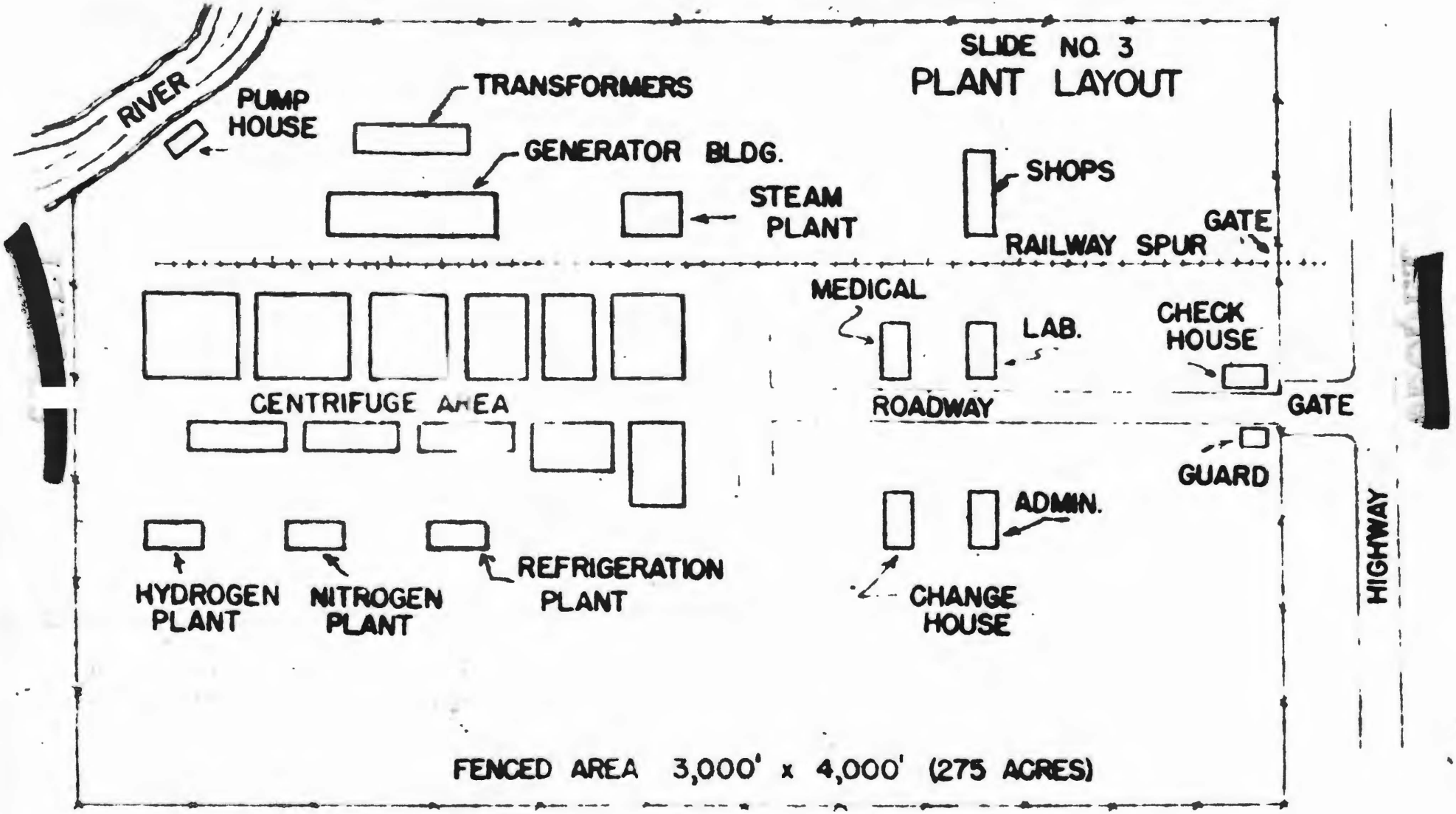
Stage No.	No. Machines in Stage	Hold-Up of Light Stream Kg.	Composition of Light Stream %
1	2,900	1.13	00.81
2	2,290	2.80	01.02
3	1,840	4.13	01.28
4	1,450	5.13	01.61
5	1,150	6.05	02.05
6	900	6.74	02.60
7	710	7.10	03.26
8	570	7.65	04.11
9	460	8.10	05.18
10	360	8.27	06.48
11	290	8.46	08.15
12	230	8.60	10.2
13	186	8.77	12.7
14	150	9.20	16.3
15	120	9.04	19.9
16	96	8.75	23.6
17	77	8.50	28.8
18	63	8.32	34.2
19	51	7.94	40.3
20	41	7.45	46.8
21	34	6.92	53.
22	28	6.39	59.
23	24	6.05	65.
24	20	5.50	70.5
25	17	5.00	75.5
26	13	4.03	80.
27	10	3.26	83.5
28	7	2.34	86.
29	4	1.38	88.5
	14,091	183.00	

STRIPPING SECTION

1	2,020	-	-
2	920	-	0.5 (outlet)
	2,940		

NOTE: The equilibrium time equivalent to the hold-up tabulated above is 124 days. If enriching stages 12 to 29 inclusive are operated at half pressure, the hold-up will be 125 kg., and the equilibrium time will be 85 days.

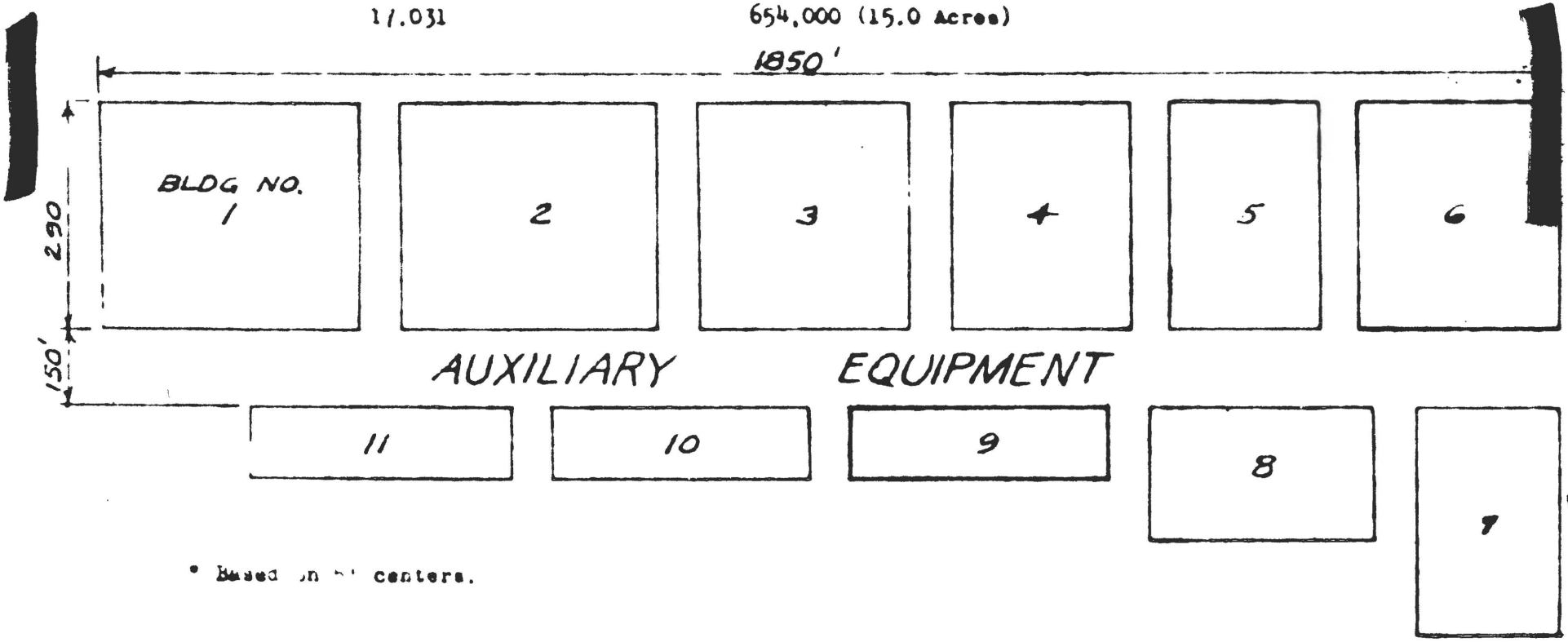
[redacted]


SLIDE NO. 3
PLANT LAYOUT
RIVER
PUMP HOUSE
TRANSFORMERS
GENERATOR BLDG.
STEAM PLANT
SHOPS
RAILWAY SPUR
GATE
MEDICAL
LAB.
CHECK HOUSE
CENTRIFUGE AREA
ROADWAY
GATE
GUARD
ADMIN.
HIGHWAY
HYDROGEN PLANT
NITROGEN PLANT
REFRIGERATION PLANT
CHANGE HOUSE
FENCED AREA 3,000' x 4,000' (275 ACRES)

SLIDE NO 4

ARRANGEMENT OF CENTRIFUGES

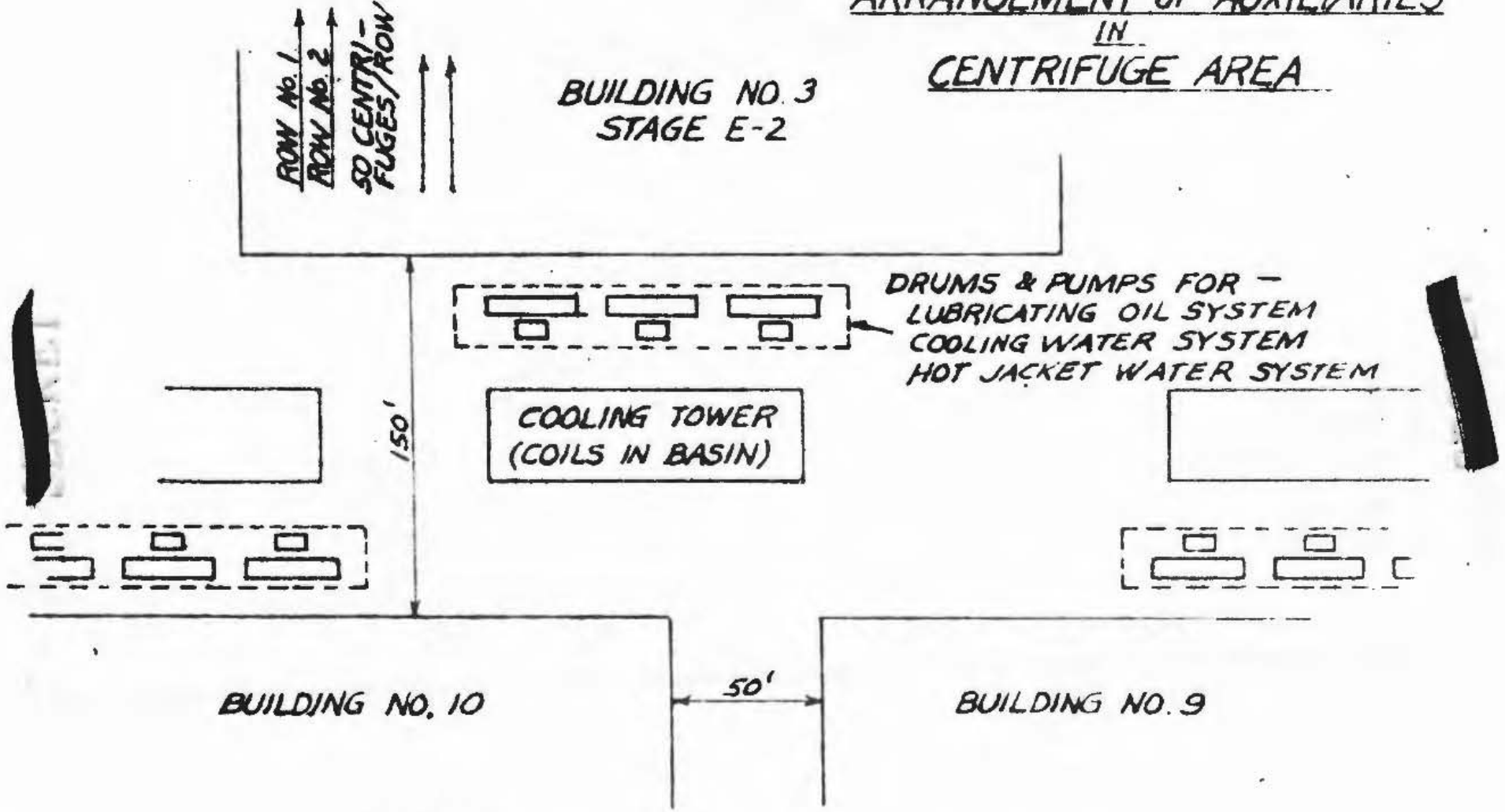
Building No.	Stages Enclosed	Total No. Centrifuges	Approx. Building Size* Dimensions	Floor Area
1	S1-S2	2,940	290'x330'	96,000 sq.ft.
2	E1	2,900	290'x330'	96,000
3	E2	2,290	290'x270'	78,000
4	E3	1,840	290'x225'	65,000
5	E4	1,450	290'x185'	54,000
6	E5-E6	2,050	290'x255'	74,000
7	E7-E8	1,280	290'x185'	54,000
8	E9-E11	1,110	165'x285'	47,000
9	E12-E14	566	90'x330'	30,000
10	E15-E20	448	90'x330'	30,000
11	E21-E29	157	90'x330'	30,000
		17,031		654,000 (15.0 Acres)



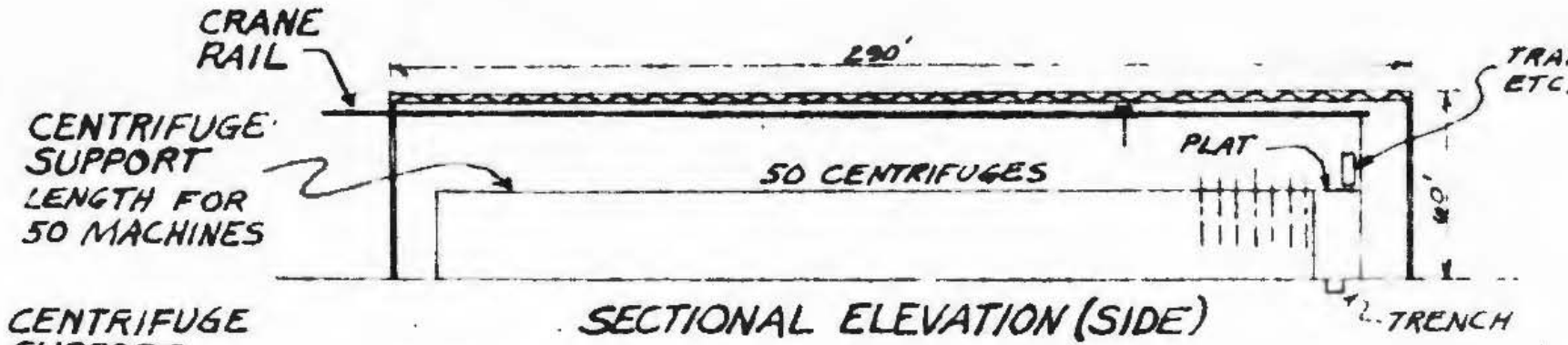
* Based on [illegible] centers.

SLIDE NO. 5

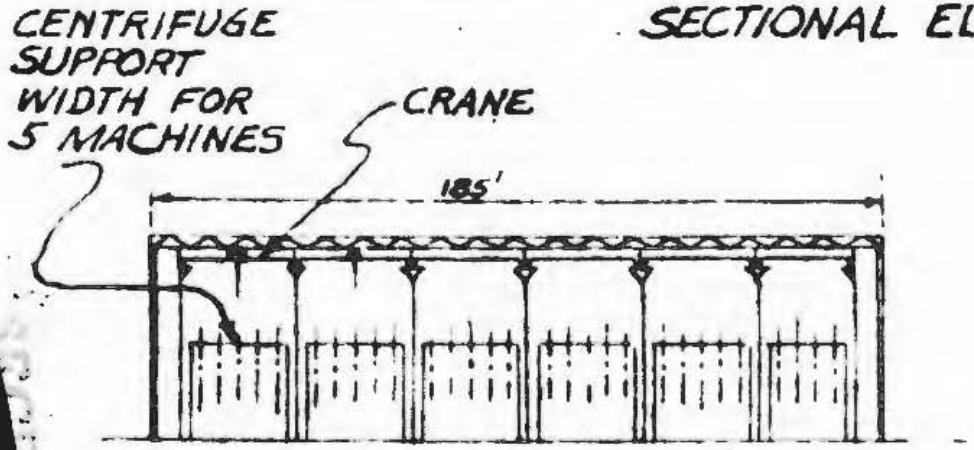
ARRANGEMENT OF AUXILIARIES IN CENTRIFUGE AREA



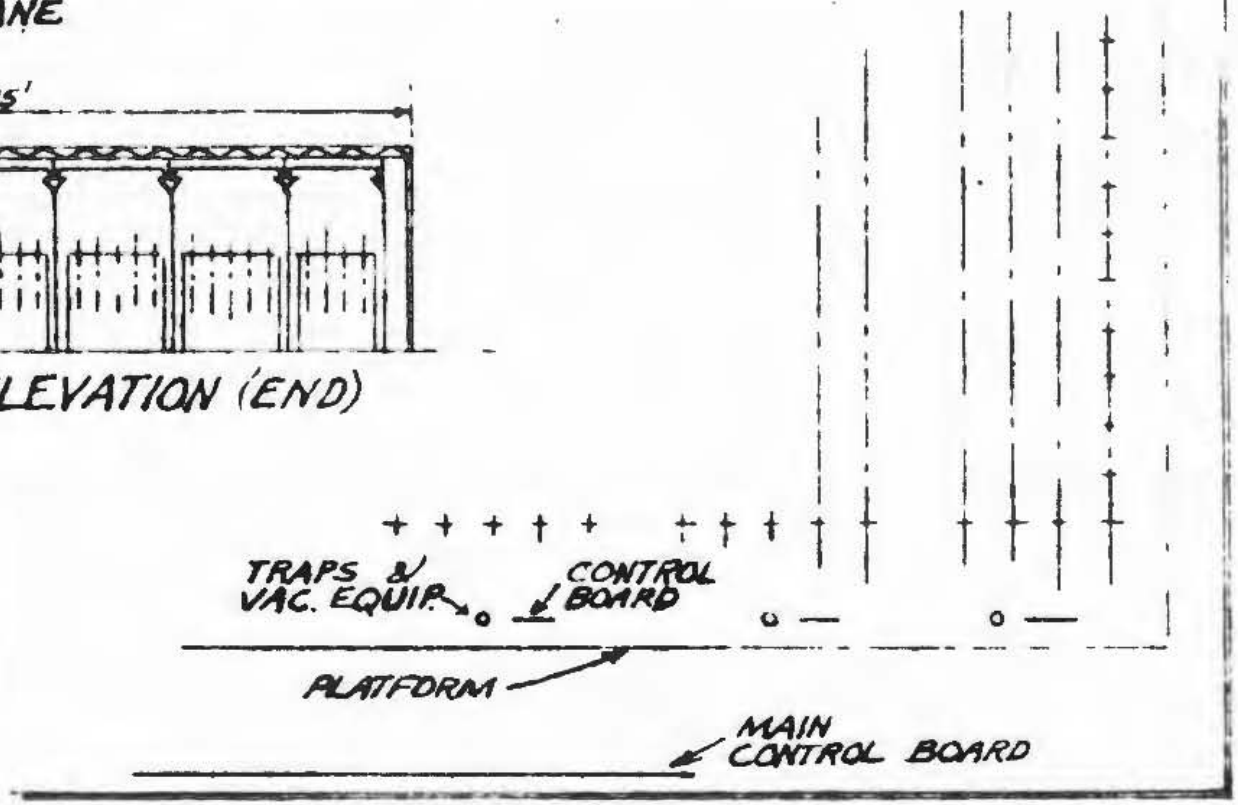
NOTE —
ALL COLD TRAPS, EJECTORS, OIL TRAPS AND OTHER SERVICES FOR INDIVIDUAL ROWS OF CENTRIFUGES ARE TO BE INSTALLED IN THE CENTRIFUGE BUILDINGS



SECTIONAL ELEVATION (SIDE)



SECTIONAL ELEVATION (END)



SLIDE NO 6

OPERATING STATIONS

[redacted]

SLIDE NO. 7

AUXILIARIES

A. LUBRICATION
- Feed Control
- Cooling
- Deaerating

B. CASING TEMPERATURE CONTROL

C. HYDROGEN
- Generation
- Feed Control
- Evacuation

D. CENTRIFUGE COOLING
- Gas Pumps
- Motor Stator

E. NITROGEN
- Generation
- Drying
- Feed Control
- Evacuation

F. LEAK-OFF
- Pressure Control
- Separation
- Return
- Evacuation

G. PRESSURE CHANGE, 11th STAGE

H. RAW MATERIAL PREPARATION
- Oxide
- Acid and Gas

I. WASTE AND PRODUCT HANDLING

J. FREQUENCY CONVERSION
- Starting Units
- Running Units

[redacted]

[redacted]

SLIDE NO. 5

CONSTRUCTION MATERIALS REQUIRED

	NET TONS
STRUCTURAL STEEL	35,000
MACHINERY STEEL AND IRON	45,000
ALUMINUM (PRINCIPALLY 14 ST EXTRUSIONS AND FORGINGS)	10,000
NICKEL (FORGINGS AND TUBING)	1,100
COPPER	
PRINCIPALLY TUBING	2,500
IN ELECTRICAL PARTS AND MACHINERY	550
CEMENT	42,000
STONE, GRAVEL AND SAND (FOUNDATIONS, ROADS, ETC.)	300,000
LUMBER	
UNTREATED	770 M Bd.Ft.
CREOSOTE TREATED	70 M Bd.Ft.
BRICKS	8,500 M

CAPITAL COST

INITIAL CONSTRUCTION	
LAND	$ 100,000
YARD WORK	1,000,000
UTILITIES, SHOPS, LAB.	4,000,000
CENTRIFUGES	23,000,000
OTHER PROCESS EQUIPMENT	10,000,000
PIPING	20,000,000
BUILDINGS	8,000,000
ELECTRIC POWER SYSTEM	7,000,000
	73,100,000
CONTINGENCY	7,300,000
TOTAL	$80,400,000
STARTING UP EXPENSE	
NON-RECURRING MECHANICAL SERVICES	$ 1,300,000
EXTRA OPERATING AND TECHNICAL SERVICES	400,000
EXPENDABLE MATERIALS	100,000
CONTINGENCY	2,200,000
TOTAL	$ 4,000,000

[redacted]

[redacted]

SLIDE NO 3

OPERATING COSTS

	Dollars/Day	
A. STAFF AND ATTENDANCE		
1. Shift Labor - All classes except Maintenance and Laboratory 225 people/shift (915 people on payroll)	6,750	
2. Repair and Maintenance Total day and shift people 300 + Materials Value	13,300	
3. Laboratory and Control - 80 people	1,000	
4. Local Management - Direct Overhead - 120 people	1,750	
		22,800
B. POWER Main Units @ 2.4 KW ea. = 40,800 KW Auxiliaries 8,060 48,860 KW @ $6/M		7,000
C. RAW MATERIALS		
1. Oxide @ $3/lb. 95% yield	4,000	
Acid @ 16¢/lb. 50% yield	200	
		4,200
2. Coal, Lub. Oil, Supplies		1,200
D. PACKAGES		300
TOTAL		$35,500

[redacted]

35

SECRET

GAS SEPARATION CENTRIFUGE PLANT

PRELIMINARY DESIGN

INTRODUCTION

At a meeting with representatives of the Keller Corporation at the Westinghouse Research Laboratories on February 24, 1943 it was agreed that the Standard Oil Development Company would investigate the possibility of using the gas centrifuge in the upper stages of a gas separation cascade.

In a subsequent discussion, Mr. Murphree agreed that consideration should be given, first, to a gas centrifuge plant design based on the production of 1 kilogram per day using feed material of 36% concentration and making product of 90%.

SUMMARY

Production of 1 kilogram of material per day by means of a gas centrifuge plant operating between concentration levels of 2% and 20% requires the use of 644 counter-current gas centrifuges having bowls approximately 11 feet long and 7.1 inches I.D. which are rotated at 470 r.p.s. The machines are arranged in 15 stages and the number of machines varies from 64 in the lowest concentration stage to 7 in the highest. Within any one stage all machines operate in parallel between the same concentration levels. The stages are connected in series and operate at the same pressure level (2 cm Hg abs.) by means of by-pass manifolds which also provide means for isolating individual stages. An allowance of 14.5% has been included in the number of machines to compensate for losses of material and mixing.

The total amount of process gas contained in the plant amounts to 140 kilograms which contributes approximately 54 days to the equilibrium time of the entire cascade.

Loss of process gas from the plant by corrosion, leakage, infiltration of air etc. amounts to 41 grams per day of material with an average composition of 58.6%. This corresponds to 1.6% of the production of light materials.

Auxiliary service requirements for the plant include the following:

1. Power: 2200 KW. - 480 cycle: 150-300 KW. - 60 cycle.
2. Lubricating & Cooling Oil: 800 GPM circulation, 75 p.s.i.g.
3. Seal Nitrogen: 1800 CFD (60°F, 1 atm.)
4. Casing Hydrogen: 10,000 CFD (60°F, 1 atm.)
5. Vacuum Exhaust: Pump capacity 1200 CFM at 0.5 cm Hg abs.
6. Casing Water Circulation: 2000 GPM, 170°F, 20-25 p.s.i.g.
7. Cooling Water: 550 GPM.
8. Recovery Traps: To recover 6750 gr./hr. process gas.
9. Refrigeration: Ethylene; for product and recovery traps; drying N$_2$.
10. Product Traps: To remove 1,65 Kg/D process gas.

a. [illegible] II.

1. Countercurrent Centrifuge Machine

The separating unit to be employed in this plant is an electrically driven, high speed gas centrifuge which is being developed at the Westinghouse Research Laboratories.

Fig. 1 shows a schematic arrangement of the countercurrent centrifuge. The bowl of the machine is approximately 11 ft. long and 7.2 inches inside diameter. It rotates within a steel casing in an atmosphere of hydrogen at 1.[illegible] cm Hg absolute pressure. Temperature control is provided by circulating water at constant temperature through a copper coil surrounding the steel casing.

Rotation of the bowl at a speed of 4[illegible] RPS is obtained by means of a 7-1/2 H.P. induction type motor whose rotor is clamped to the upper shaft. The entire rotating assembly is supported on a thrust bearing below the end of the lower shaft.

The shafts are made of hollow dual passage tubing (3/4" O.D.) to conduct the process gas to and from the bowl. Movement of the gas is accomplished by means of a pump which is clamped to each shaft.

Five journal type bearings are located along the shafts for positioning and two oil damper bearings are provided to take up vibrations. The journal bearing below the motor acts as the vacuum seal for the machine since below this point pressures of about 2 cm. Hg absolute will exist.

Two water cooled pump housings are provided, one on each shaft. As shown in Fig. 2, these housings contain the pump and process gas passages which connect with the rotating shaft. Seals are required to segregate the process gas and the lubricating oil. These are shown in Fig. 2 on each side of the openings to the shaft. It is planned to use bone dry nitrogen as the medium for keeping the process gas separated from the lubricating oil. A certain amount of process gas leakage will occur in the seals and a recovery system must be provided to return this material to the system.

Distribution of the gas within the rotating bowl is to be accomplished by means of end caps containing radial slots or holes. The heavy gas is conducted to the periphery of the bowl through these radial slots and flows down the inside wall

FIG-1

SCHEMATIC ARRANGEMENT OF COUNTERCURRENT CENTRIFUGE

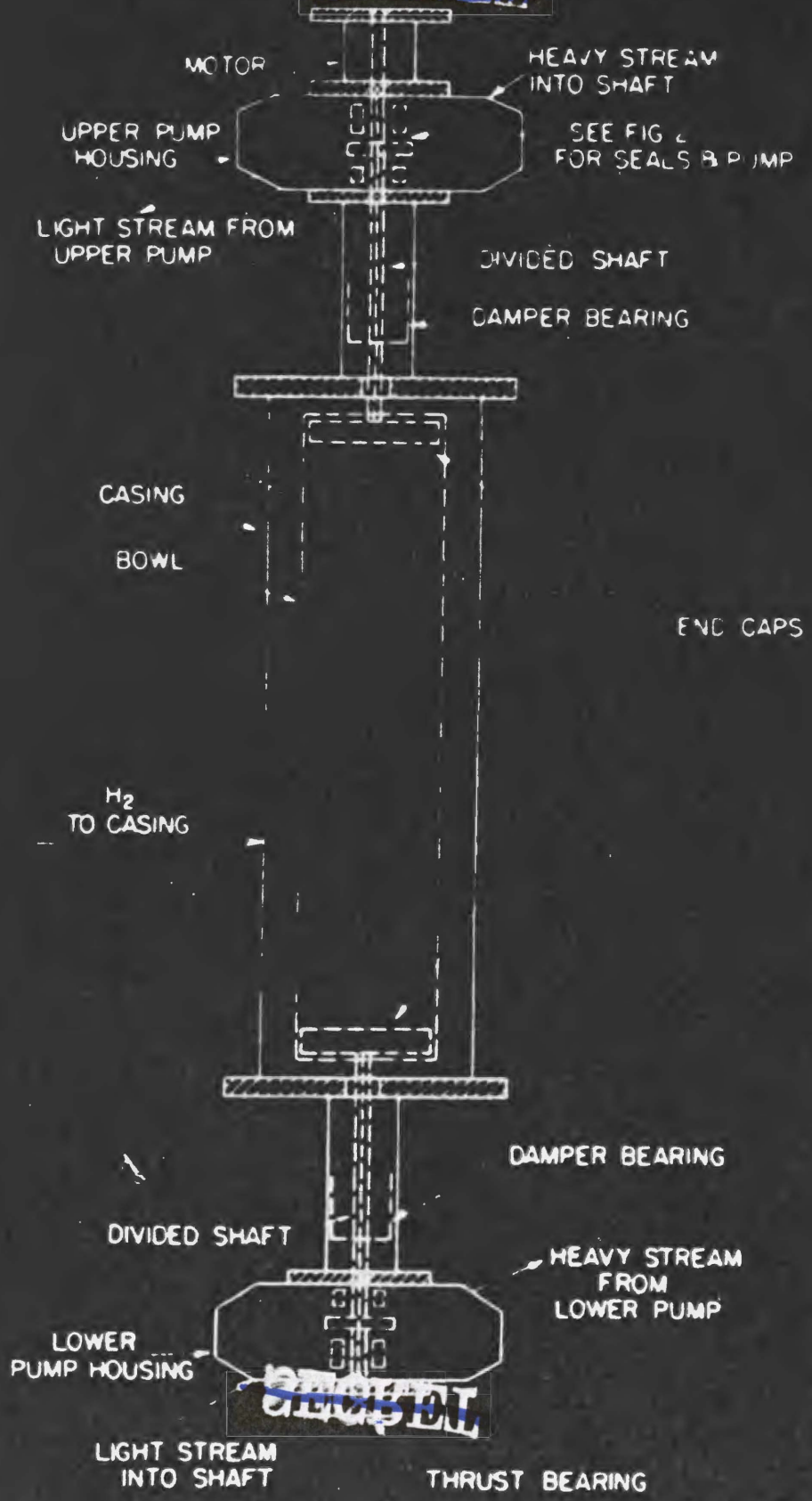
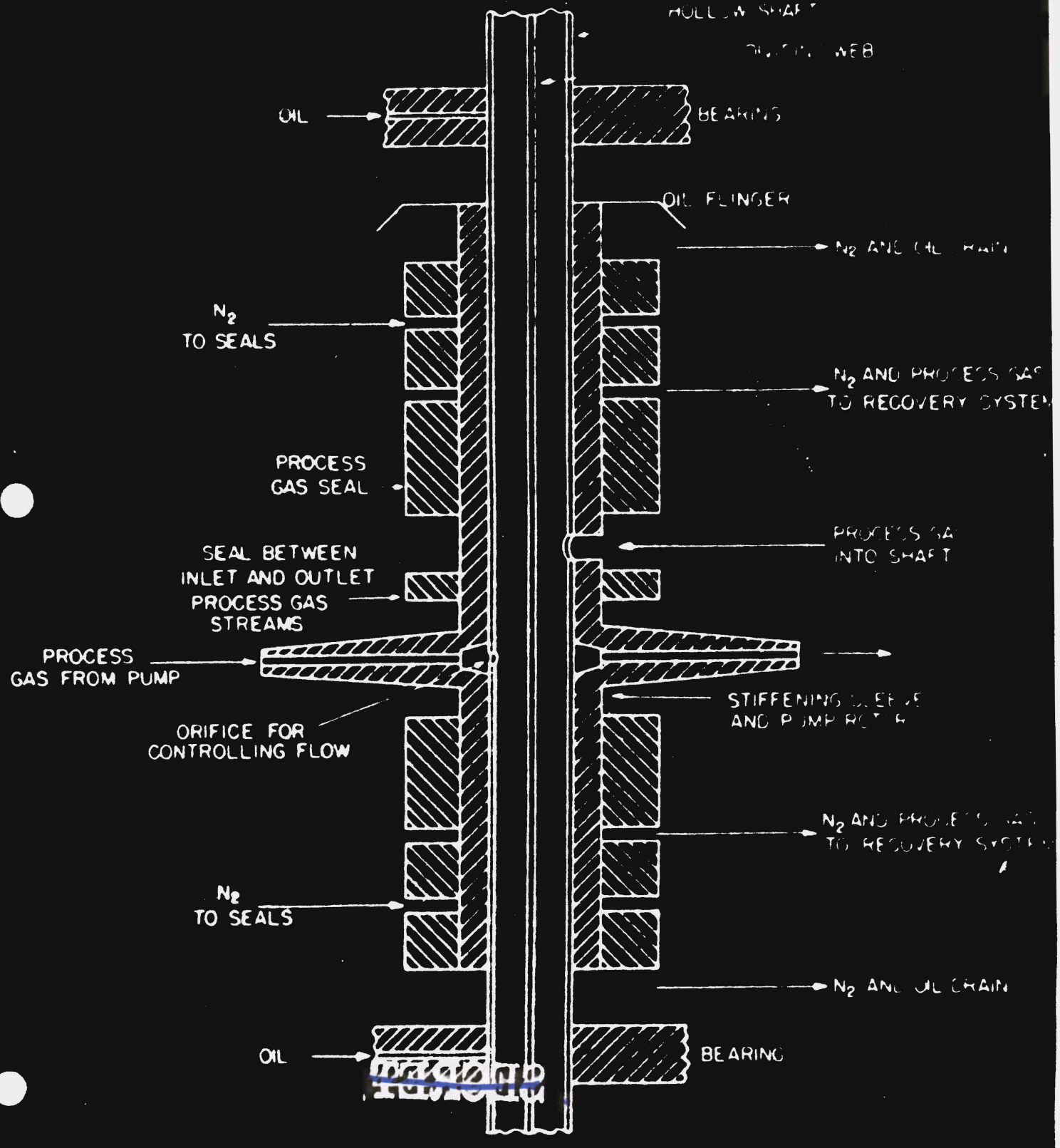


FIG.-2

SCHEMATIC ARRANGEMENT
OF
UPPER SEALS AND PUMP



in an annular path. It is returned to the lower shaft through radial slots in another cap of similar construction.

The light gas flows upward, countercurrent to the heavy. Radial holes in the lower cap introduce this gas in an annular stream approximately half way out to the wall of the bowl. It is returned to the upper shaft through similar radial holes in the upper cap. Each machine weighs approximately 3000 lbs. and has an overall length of 18 ft.

2. Plant Arrangement of Machines

A kilogram per day plant operating between 3% and 90% concentration levels requires the use of 624 machines arranged in 15 stages as shown in Fig. 3. The machines in each stage are all in parallel while the stages are all in series.

Floor space of 20,000 sq.ft. would be required for the machines alone with a center-center spacing of 6 ft. The building required for machines grouped into 15 stages would have about 40-50,000 sq.ft. of floor space.

3. Process Manifolds

The arrangement of the process manifolds is shown in Figs. 4 and 5. Figure 4 shows the arrangement of the piping within a stage. Valves are provided so that each row of machines within the stage may be isolated.

Fig. 5 shows the process manifolds which connect stages. Valves are provided for isolating and bypassing any stage.

Each stage is provided with two continuous type recovery traps in order to keep the seal leakage occurring at opposite ends of the machine from mixing.

The tie-in between the centrifuge plant and the plant below is shown below the lowest stage in Fig. 5. It is assumed that the lower plant would be operating at a higher pressure than the centrifuge plant and in order to mix the streams at this point a pressure control valve and a pump, together with a control valve are provided.

4. Operating Conditions and Flow Control

At an operating speed of 470 RPS and process gas manifold pressures of 2 cm Hg absolute, throughout the entire centrifuge plant, the temperature of the bowls must be maintained at about 170°F in order to prevent condensation.

Gas Separation Centrifuge Plant

Machines and Auxiliary Services.

Basis: 1 kilogram per day.

624 Machines

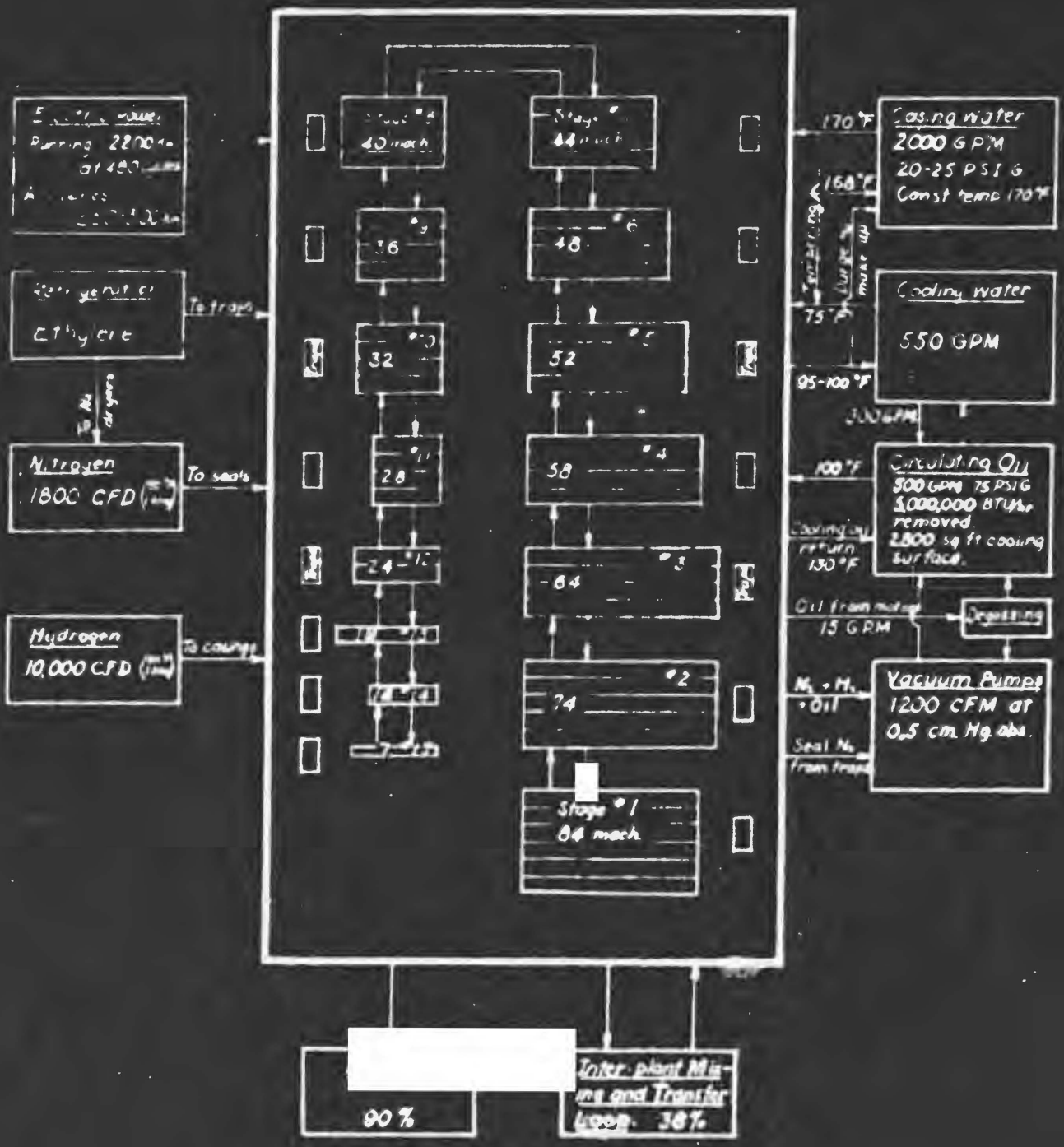


Fig. 3

Arrangement of Single Stage
of Machines

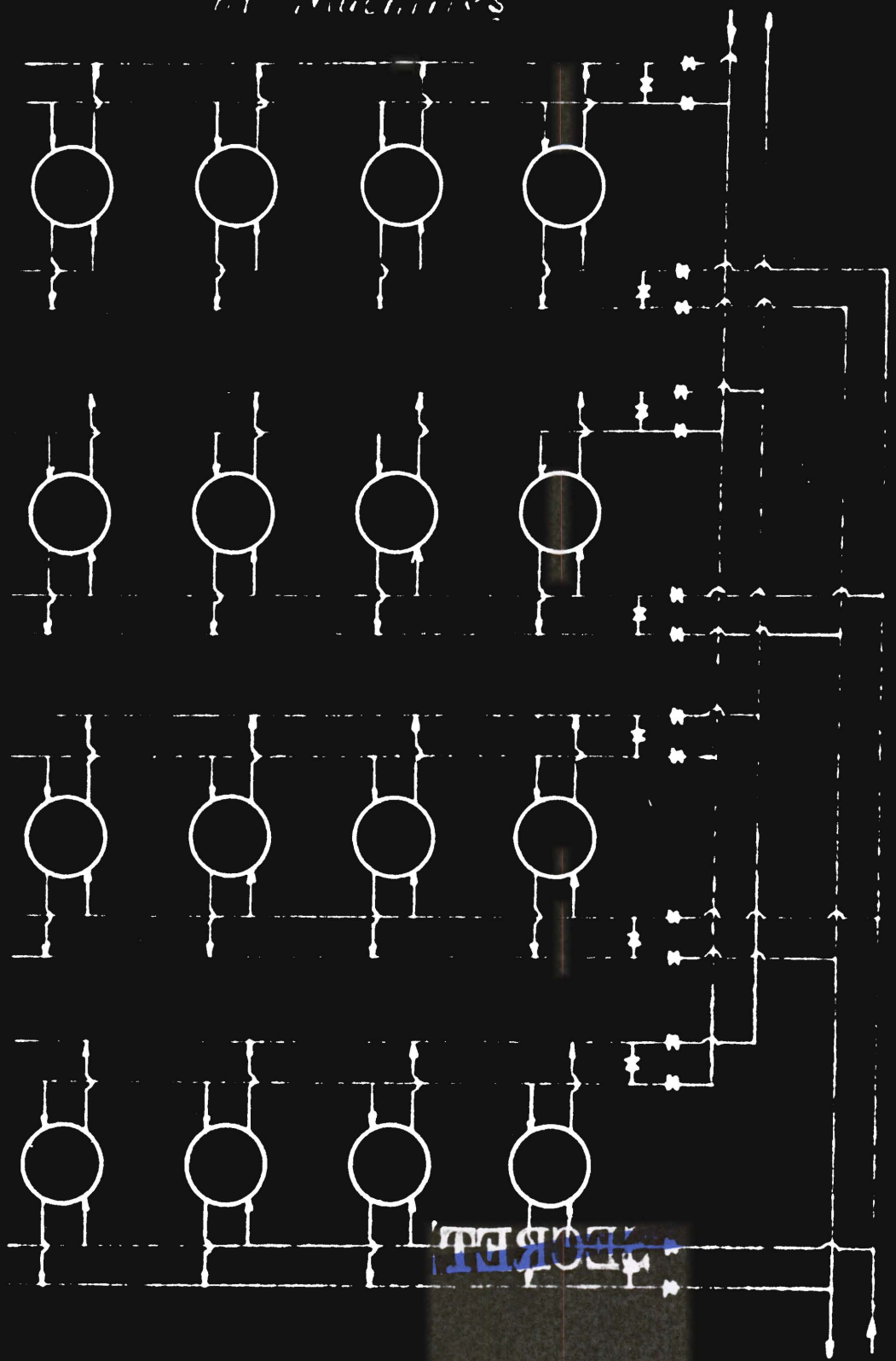


Fig. 4

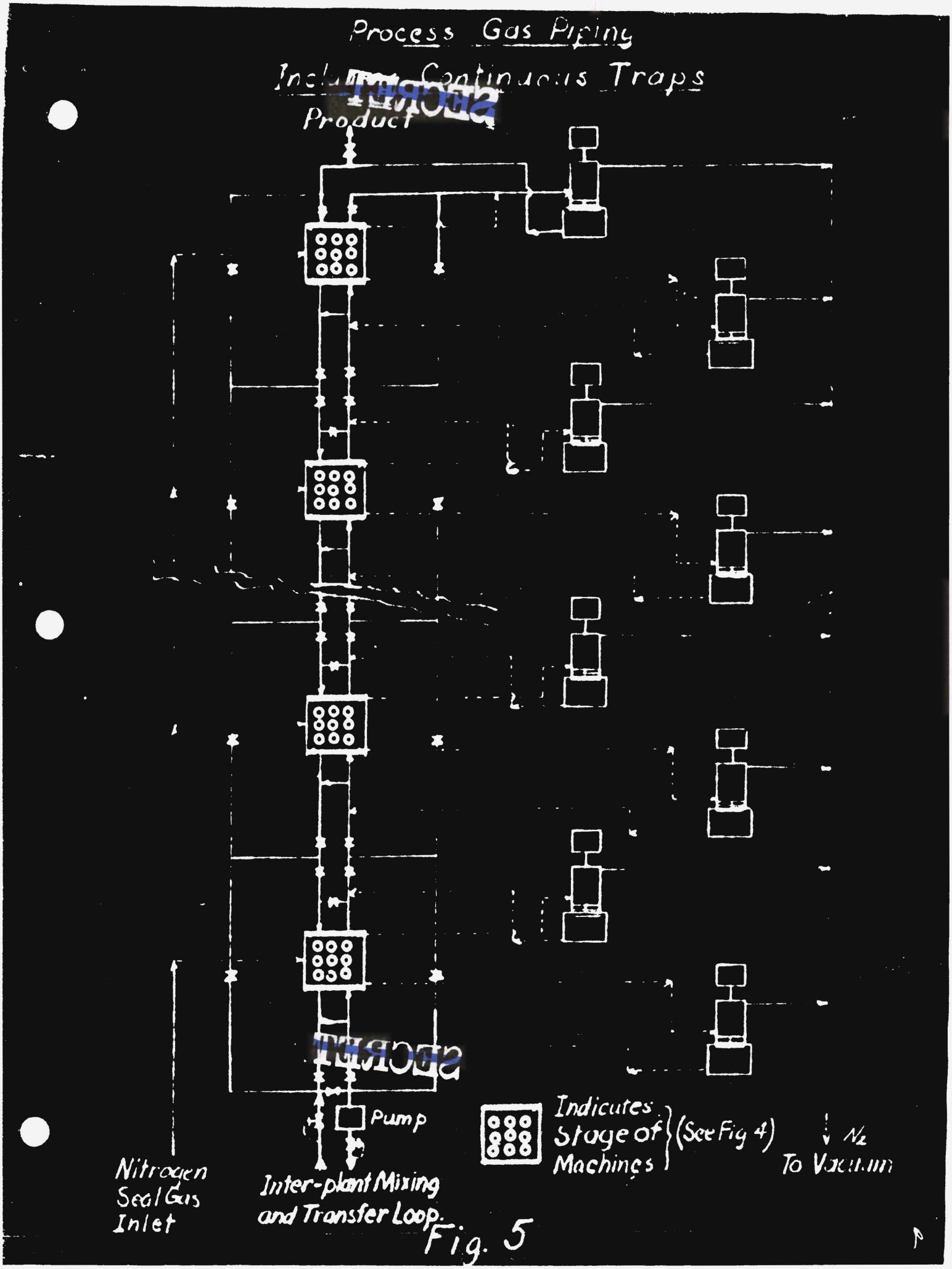

Process Gas Piping
Including Continuous Traps
Product
Pump
Indicates Stage of Machines
(See Fig 4)
N_2 To Vacuum
Nitrogen Seal Gas Inlet
Inter-plant Mixing and Transfer Loop.

Fig. 5

Each centrifuge is provided with four orifices with the shafts to assure proper distribution and magnitude of flow in all machines.

Forward flow equal to the product withdrawal rate, together with any unequalities of flow between the machines, takes place in the two vertical bypass lines shown in Fig. 5. Alternate valves in these lines will normally be open so that a continuous path exists between the bottom and top of the plant. To compensate for mixing due to the use of these bypass lines, four additional machines are included in each stage.

5. Auxiliary Services

In order to insure proper operation of the centrifuges the following auxiliary services must be provided:

1. Power
2. Lubricating and cooling oil.
3. Dry seal gas (nitrogen)
4. Casing gas (hydrogen)
5. Vacuum exhaust system.
6. Casing water (constant temperature)
7. Pump and motor cooling water.
8. Recovery traps.
9. Refrigeration (ethylene, -150°F)
10. Product trap.

The requirements for each of these services are outlined in Fig. 3 and will be discussed below in more detail.

B. BASIS FOR PROCESS DESIGN

1. Number and Arrangement of Machines

The process design of a cascade of centrifuges depends on the determination of two quantities; the number of theoretical plates per centrifuge, n, for a given flow rate and the simple process factor, 1 + B. Both of these quantities are functions of the diameter, length and rotating speed of the bowl and of the flow pattern within the bowl. They may be established theoretically for a certain assumed flow pattern or they may be established by experiment.

It is proposed at present to use a bowl with an effective length of 126" (3.2 meters), and a diameter of 7.2". This bowl will be operated at a rotation speed of 470 RPS and a temperature of 170°F. For the purpose of calculating the theoretical operation of this bowl, it is assumed that the flow will take place in two countercurrent, infinitely thin annular streams, one at a radius of 1.9" and the other at the periphery. The flow in each stream will be 4 k_g/D. For these conditions the theoretical number of plates would be n = 12." and the theoretical simple process factor would be B = .0274.

At present, some experimental data at low product rates are available from the University of Virginia on a centrifuge operating as a single refluxing unit. Using the calculated value of B = .025 and taking the difference between theory and experiment as a correction on n, the number of plates, the "plate efficiency" is about 70%.

Date - 1943	Jan. 2	Jan. 5	Jan. 27	Feb. 3
Flow in each stream - m_g/sec.	33.3	28.6	35.2	38.6
Product rate - m_g/sec.	0.77	0.41	0.47	0.47
B	.025	.025	.025	.025
n_o (number theo. plates)	8.38	7.90	7.14	6.60
(N_o/N_z) max.	1.102	1.150	1.103	1.094
n (actual number plates)	4.33	5.25	4.26	3.82
Plate Efficiency = n/n_o	.522	.740	.598	.554

Incidentally, the runs were made on a bowl of diameter 7.80 cm. and length 81.3 cm, rotating at a speed of 1020 RPS. The temperature was 70°C.

In using these data for design some assumption had to be made as to the specific effect of the inefficiency on n and B. Since n and B are both functions of the flow pattern within the bowl, it seemed reasonable to divide the variation between them. On this basis the value of n for the large bowl would be 15.9 and the value of B would be 0.0287. The general process design was therefore worked out on this basis. Fig. 4 shows the general layout of the machines. Some allowance has been made for certain losses of efficiency which will be discussed below. The following table summarizes the design:

Stage	No. Machines	Total Holdup Kg	Avg. Composition
1	64	17.6	38.2
2	64	15.6	42.7
3	64	13.4	47.3
4	56	12.2	51.6
5	52	10.9	56.3
6	48	10.1	60.9
7	44	9.2	65.2
8	40	8.4	69.1
9	36	7.6	72.6
10	32	6.9	75.4
11	28	6.2	78.0
12	24	5.6	78.5
13	18	4.6	80.6
14	14	2.9	77.2
15	8	1.5	69.1
	524	130.9	58.8

The holdup of material and resulting contribution to the equilibrium time are as follows:

	Holdup Kg	Contribution to Equil. Time, Days
Centrifuge Bowls	130.9	51
Piping	1.35	0.5
90° Traps (2 different sizes)	7.5	2.9
	139.8	54.4

2. Losses of Efficiency

The following losses in efficiency due to cascade design and operation have been considered:

1. Corrosion loss.
2. Leakage of air into system.
3. Leakage of process gas out of bowls.
4. Back diffusion of process gas through buffer seals.
5. Incomplete recovery of process gas by continuous traps.
6. Inequalities between upflow and downflow in machines in parallel in one stage.
7. Bypassing from one manifold to the stage ahead.
8. Mixing of seal gas outflow from points of different composition.
9. Loss due to flow between upflow and downflow manifold at each stage.

Items 1 through 5 involve losses which would affect the size of the base plant required. The remaining items affect only the size of the upper plant. These items will be discussed in the following paragraphs numbered to correspond with the items above.

1. A corrosion destruction rate of 2 mg/sq.ft./day was assumed for all metal surfaces in contact with process gas. The total area exposed has been calculated as follows:

11,000 ft. of 1" tubing	2880	sq.ft.
4,400 ft. of 1 1/2" "	575	"
2,000 ft. of 2" "	1050	"
500 ft. of 1-1 1/2" tubing	130	"
824 centrifuge bowls	12,000	"
Pumps for above	1,000	"

Total area, say, 18,000 sq.ft.

Using the above rate of destruction, the loss would be 36 gm/day of material with an average composition of 50%, or about 1.4% of the net production of light material.

2. Tests now in progress at the Westinghouse Research Lab. indicate that leakage through hard soldered joints of the type contemplated for the process gas system will be less than 8×10^{-12} mol/sec./joint. Assuming 100 joints per machine, the total leak of air for the plant will be 0.032 mol/day. Assuming that this air will contain 5% water which would react mol for mol with process gas, the total destruction of process gas would be about 0.58 gm/day. Tests on the

end cap gasket for the bowl have shown that a total leak rate for the two end cap gaskets of 10^{-10} mol/sec. with a pressure difference of 1 atmosphere can be readily attained. Assuming that the 14 half inch process connections (gasket type) to the machine leak as much as the two end cap gaskets, the total leakage of air into the system would be 0.005 gm. mols/day. This would destroy about .09 gm/day of process gas. Thus the total destruction by leakage of air into the system would be about 0.85 gm/day or about .03% of the net light production.

3. At the above rate of leakage, the amount of process gas escaping from the bowls (since the gas at the periphery of the bowls will be at approximately atmospheric pressure and the bowl spins in low pressure hydrogen) will be 1.9 gm/day or 0.075 % of the net light production.

4. Assuming a seal clearance of 0.002" radially, a seal diameter of 1.5" and length of 1/2", the amount of diffusion of process gas against an inflow of N_2 from 2.0 cm to 1.5 cm Hg will be

$$Q = \frac{S}{e^{\frac{RTSL}{D_P}} - 1} A = 21.3 \times 10^{-18} \text{ gm/sec./seal}$$

Since there are 4 seals per machine, the total diffusion of process gas through the seals will be 4.05×10^{-9} gm/day or a negligible quantity. Since this quantity is so small, it seems unnecessary to consider the departure of actual seal conditions from the ideal condition assumed in deriving the above formula.

5. The amount of nitrogen associated with the process gas going to traps is estimated at 900 CFD (60°, 1 atm.). Assuming that the traps operate at 1.5 cm pressure and that the stream is cooled to -145°F (V.P. of process gas = 1.2×10^{-5} cm), the loss of process gas will be 2.1 gm/day or 0.08% of the net light production.

6. The fractional loss of separating power of a countercurrent centrifuge due to the fact that the two streams are not correctly balanced has been shown (W.I.T. Memo, March 3, 1943) to be approximately

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$$\text{fractional loss} = \frac{\epsilon^2 N^2}{4}$$

where ϵ is the fractional departure of the ratio of upflow to downflow from the ideal value and N is the number of plates per centrifuge (15.3 in the present case). The flow control system for a centrifuge consists of a set of orifices, one in each inlet and one in each outlet, all of which operate at accoustic conditions. In addition, there is a small pressure drop due to friction in each shaft. Each dual passage shaft is made up of a pair of concentric tubes. The effects of variation in dimensions of the shafts and orifices are summarized in the following table:

	Inlet Shaft	Outlet Shaft
Orifice Diam., inches	0.10	0.20
Tolerance, inches	±0.0002	±0.0002
Flow variation, %	0.80	0.40
Shaft friction drop, mm Hg	0.83	0.3
% of inlet pressure	3.94	3.75
Inner shaft, I.D."	0.250	
Tolerance, inches	±.001	
Flow variation, %	0.12	
Inner shaft, O.D."		0.300
Tolerance, inches		±.002
Flow variation, %		0.136
Outer shaft, I.D."		0.600
Tolerance, inches		±.001
Flow variation, %		0.084
Total flow variation, %	0.92	0.92

The inlet and outlet flows are not free to vary independently and errors in one flow stream tend to reduce the errors in the other streams. Assuming the errors all at the maximum tolerances shown in the table above and arranged to supplement each other, the greatest percent difference between the design flow ratio and the actual flow ratio is

$$\frac{0.92 + 0.92}{2} = 0.77\%$$

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The corresponding percent loss of separating power is 0.3%. The above tolerances do not seem excessive. However, the resulting loss of separating power is so small that these tolerances could be somewhat relaxed if necessary.

7. The loss of efficiency due to bypassing from one stage to the next will depend on the cascade design. With the open bypass arrangement (Fig. 5) an amount equal to the product will bypass across each stage. This represents 10.9% loss of separating power and requires approximately 4 extra machines per stage. This correction has been made.

8. Since the seal gas quantities are small, and it is proposed to use separate trapping systems for each stage, this loss of work should be completely negligible.

9. Since the cascade is progressively reduced in size toward the top, a certain amount of the forward flow from each stage must be bypassed to the downflowing stream. In addition, if the seal leakage is all returned to the same point, an additional flow between these streams takes place. Assuming the maximum amount of this flow, the loss in efficiency throughout the cascade amounts to about 0.7%.

Summarizing the discussion of losses, the actual losses of process gas due to the first five items amounts to about 28 + 0.7 + 1.9 + 0 + 2.1 = 41 gm/day, or 1.4% of the light production. This loss increases the size of the plant* by about 0.5%.

The losses of separative work from other causes (affecting only the size of the centrifuge plant) amount to about 0.4 + 10.9 + 0 + 0.7 = 12%. The total correction to be applied to the size of the centrifuge plant is therefore 12.5%.

*Centrifuge plant only. The base plant will be increased in direct proportion to this loss.

C. AUXILIARY SERVICE REQUIREMENTS

1. Power

The major power requirement in the plant is that necessary to maintain the rotational speed of the centrifuges. Under running conditions it is estimated that each machine requires 3.5 KW at 480 cycles per second. This is equivalent to 2200 KW for the 624 machines.

The basic requirements for this power are continuity and precise frequency control. These considerations, together with the relatively small amount of power involved make it desirable to consider the direct generation of this amount of power at 480 cycles per second for the centrifuge plant.

The power required for the auxiliary services is possibly 10-15% of that required by the machines. Rough estimates of the various electrically driven services are:

Casing water circulation	45 KW
Cooling water circulation	20 KW
Oil circulation	35 KW
Vacuum pumps	55 KW
	155 KW

The power and refrigeration requirements of the seal recovery traps have not been estimated since they will depend upon the design of the traps and the piping layout. An emergency source of electric power must be provided for practically all of the auxiliary services in order that these installations may be kept in operation during power failure.

2. Lubricating and Cooling Oil

Each machine is provided with 45 GPM of oil at approximately 100°F and 50 psig for both lubrication and cooling of the bearings. The total oil circulation for the plant is approximately 500 gpm. This requires pumps, filters and coolers for the removal of 5 M. BTU/hr. The cooling surface requirement is about 2800 sq.ft. and the cooling water necessary is about 300 gpm. It is essential that the oil pressure be maintained whenever the machine is in operation.

Of the above oil circulation, approximately 15 gpm comes in contact with the atmosphere at the motor bearings, and consequently provision should be made to degas this oil

by means of a degassing tower before returning it to the circulating system.

The oil piping should be arranged so that each stage will be supplied as a unit from the main manifold.

3. Dry Seal Gas

A continuous supply of 1800 CFD (60°F, 1 Atm.) dry nitrogen is fed to the gas seals at about 2 cm Hg absolute. This pressure will be anchored to that in the process gas manifolds by a differential pressure controller. The nitrogen will be dried by chilling with ethylene.

The seal nitrogen piping probably should be arranged so that each stage will be supplied as a unit from a common source.

4. Casing Gas

In order to reduce the power requirement and facilitate the removal of heat from the spinning bowls, hydrogen is admitted to the casing which is held at about 1.5 cm Hg absolute; 10,000 CFD (S.C.) will be required for the 624 machine casings. The hydrogen will be distributed to each stage at 10-15 psig from a common manifold. Within each stage distribution to each machine is accomplished by capillary tube meters.

5. Vacuum Exhaust System

All of the nitrogen and hydrogen which enter the system must be removed by the vacuum pump. The nitrogen-process gas mixture passes through the seal recovery traps before entering the exhaust system. The nitrogen and casing gas, both of which are mixed with oil from the bearings, pass through oil traps before entering the exhaust system. The pressure of the exhaust system will be held at about 1.5 cm Hg absolute by a differential pressure controller which is tied into the process gas system.

The exhaust piping from the machines in each stage will be grouped so that the entire stage can be isolated from the rest of the system. It is estimated that vacuum pumps having a capacity of 1200 CFM at 0.5 cm Hg absolute will be sufficient to remove all the nitrogen and casing gas.

6. Casing Water

In order to maintain each casing at a constant temperature (within 1°F) 120 GPM of water at 170°F must be supplied to coils surrounding each casing. This corresponds to 1250 GPM for the entire group of machines. In order to take care of heat losses in the casing water distributing manifolds an additional 750 GPM should be circulated through these manifolds, thereby giving a total of 2000 GPM.

Since the distribution of water amongst the various casings will be made by orifices, the circulating pump should be capable of delivery at 20-25 psig.

Means for maintaining the circulating stream at constant temperature must be provided.

The piping should be arranged so that each stage can be isolated from the circulating system.

7. Pump and Motor Cooling Water

The two pumps and motor on each machine will require 24 GPM of cooling water making a total for the plant of 250 GPM. This water should be tempered to about 75-80°F by bleeding off a proper amount of casing water. Casing water purge and make-up can in turn be obtained from the cooling water leaving the machines.

An additional 300 GPM is required for cooling the circulating oil, making a total of 550 GPM for these services.

The pump and motor cooling water piping should be made so that each stage may be supplied as a unit from a common source.

8. Recovery Traps

Under the contemplated operating conditions it is estimated that 3 mg/sec. of process gas will leak out through the seals in each machine. This gas, which is mixed with small nitrogen must be continuously recovered and returned to the system at a point close to its origin.

Work is now in progress on the development of a continuous scraped surface type recovery trap employing liquid ethylene as the refrigerant. The construction of the trap is

such that all cold surfaces are scraped, thereby insuring low holdup. A powder-seal is used to remove the condensed process gas from the bottom of the trap. It is then sublimed from a heated section below the powder-seal and returned as gas to the system.

Each stage will have two recovery trap systems, one for the seals on the upper part of the machines and one for the lower seals as outlined in Fig. 5. The total process gas which must be recovered amounts to 4750 gms/hr. The amount in the base stage is 910 gms/hr. and the amounts in remaining stages are proportional to the number of machines in the stage.

It is also planned to employ a continuous trap on the entire material leaving the top stage in order to remove any nitrogen or air that exists or has leaked into the system.

9. Refrigeration

The minimum refrigeration requirement for drying the seal nitrogen and for chilling the gases in the recovery traps amounts to only 325 and 1200 BTU/hr., respectively. The traps will undoubtedly require considerably more because of heat losses and the recirculation through the powder seal. These losses, together with the losses in the refrigerant piping, will constitute the major load on the system.

In order that the loss of process gas from the recovery traps be kept at a reasonably low amount, it is necessary to cool to temperatures of approximately -150°F. This may readily be accomplished by the use of liquid ethylene as the refrigerant.

Since a refrigerant of this type must be employed, it also presents a convenient method for drying the seal nitrogen.

10. Product Trap

A batch trap system will be used to remove product from the plant. This trap system would use the same refrigerant as the seal recovery traps and it represents only a small additional load on the refrigeration system.

March 15, 1943

[redacted]

OFFICE FOR EMERGENCY MANAGEMENT

OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT

1530 P STREET NW.

WASHINGTON, D. C.

VANNEVAR BUSH
Director

[illegible] roadway
[illegible] New York 4, N.Y.
[illegible] December 6, 1943

Dr. L. J. Briggs
Dr. A. H. Compton
Dr. J. B. Conant
Dr. E. O. Lawrence
Dr. H. C. Urey

[redacted]

Gentlemen:

I am transmitting for your information a letter from Dr. Paul E. Kuhl with accompanying memorandum by Messrs. Belden, Russell and Gillespie, reviewing the progress that has been made in the gas centrifuge pilot plant.

Two small centrifuges having 36-inch bowls have been operated at the pilot plant. The first machine run, which was somewhat of an improvised job, had two operating periods - one of 14 days and one of 10 days, making a total of 24 days. The first operating period was terminated in order to make certain changes in the instruments; the second operating period was terminated to install the second short bowl machine which contained certain mechanical improvements. During the period of 33 days of mechanical operation of the centrifuge, process gas was run through the machine for a total of 18 days.

The second short bowl machine was put in operation October 9 and was run for 17 days, at which time the run was terminated due to a faulty valve leaking air into the casing of the centrifuge which caused an overload in the driving motor. The second operating period on this centrifuge of 12 days was terminated due to the lower gas pump either being improperly installed or slipping during operation which resulted in seizure of the pump. This pump was held by a friction device and the construction here can undoubtedly be improved. The third operation period, which is still continuing, has now extended over 23 days. On the second machine the period of operation on process gas is now about 45 days.

Separation results obtained on the centrifuges have been very satisfactory, a cascade efficiency of from 75-79% having demonstrated. By cascade efficiency is here meant the comparative work actually obtained by the centrifuge compared to the theoretical optimum. Based on the separation efficiency obtained it is estimated that a large plant to produce 1 kilogram per day of light material of 90% purity will require slightly over 21,000 long-bowl machines in the enriching section. Approximately 7,000 additional machines will be

[redacted]

required in the stripping section to obtain 50% recovery of light material. The power consumption of the roughly 28,000 machines is estimated to be around 150,000 kilowatts.

Up to December 1 a total of 500 lbs. of process gas has been handled by the centrifuges. Of this material about 400 lbs. has been discharged as waste which has been depleted in the lighter component by roughly 1%. The remaining 100 lbs. has been discharged as product with an average enrichment of about 4%.

The pilot plant is operated with three nontechnical operators per shift, the main work involved being simply handling of cold traps for the process gas. The same group undoubtedly could handle a much larger number of machines, and if traps are used not requiring labor for handling one operator could run the whole plant with a considerably greater number of centrifuges than are now installed.

Unless we are advised otherwise by O.S.R.D., we are at present planning to discontinue operation of the pilot plant December 31, 1943. Actually it will be necessary to start shutting down a few days before this time. It is felt that the centrifuge operation looks extremely promising and it is our feeling that it would be wise for the development to be continued. If it is continued, however, it is felt that work should again be initiated on fabrication of a long-bowl centrifuge as such a machine will be required for large scale application of the process.

The writer is not familiar with recent developments on the diffusion project but it is understood that difficulties of a rather fundamental character are being encountered. Dr. Urey or Mr. Keith can undoubtedly furnish the committee further information if they so desire.

Due to the considerable progress that has been made on the centrifuge development, and the definite experimental demonstration of the feasibility of this operation, it is felt that the project should carefully be reviewed by either O.S.R.D. or the Army prior to its being abandoned. It is suggested that Dr. Conant may wish to refer this question to General Groves.

Very truly yours,

P. C. Murphree

K. V. MURPHREE

EVM:DF
Enclosure
Copy to Brig. Gen. Leslie R. Groves /

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* Page 1, cover sheet, has not been included.

December 7, 1943

Brig. General L. R. Groves
P. O. Box 2610
Washington, D.C.

Dear General Groves:

As I told you early last week, it seems to me that the centrifuge pilot plant development at Elizabeth, should be taken very seriously. I have accordingly arranged a table showing comparative figures on some characteristics of a postulated centrifuge plant and the diffusion plant. None of these figures are highly precise, but they do, I believe, indicate order of magnitude.

I wish especially to direct your attention to the difference in the areas exposed in the process gas, and the consumption rate calculated on the basis of our measurements, making no allowance for any scouring action in the high speed pumps of the diffusion plant. The consumption of the diffusion plant is 15 times that calculated for the centrifuge plant.

The total amount of pumping in the centrifuge is very much less than in the diffusion plant, and the amount and sizes of piping required are very different, as you will see.

The extent of pilot plant operation in the two cases is also very different. The size of the apparatus for the centrifuge pilot plant is 220 times that of the diffusion pilot plant, and has run much longer times. Also the Bayway pilot plant for the centrifuge is of the type that would be used in the final plant except that the bowl is a 3 foot bowl instead of a 10 foot bowl; while for the diffusion plant the small pilot plant that we have constructed is not of the type that would be used in the plant at all. Not for some months yet will we have any pilot plant operating that will give information on the actual behavior of the diffusion plant, and this pilot plant will have dummy diffusers.

The question of going from the 3 foot bowl to the 10 foot bowl is one which will disturb you, but I believe that this is a minor extrapolation as compared with the ones that must be made on the diffusion plant. The 10 foot bowl

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has been operated at Virginia for short periods of time without difficulty, and it does seem to me that we can rely upon the Westinghouse estimate of what such a bowl will do. The results at Virginia show that the long bowl produces the same amount of separative work per foot as the short bowl operating under comparable conditions.

A cost figure is given which is based upon the estimate made by a committee from the Kellex Corporation and the Standard Oil Development Company last spring, for the purpose of comparing the two types of operation for the top part of the plant. It should be as accurate as the estimates made for the 624 machines on which the estimate was based, since the entire centrifuge plant would be a repetition of one type of machine.

The centrifuge plant, so far as development is concerned, is, in my mind, far ahead of the diffusion plant. We do not have a satisfactory barrier, even in the laboratory, and our tests on consumption of process gas are made on comparatively small samples. We know nothing whatever about the plugging of barriers by dust. The conditioning of the diffusion plant, and the exclusion of small amounts of moisture are very difficult, and comparisons are all favorable to the centrifuge system. The size of the apparatus that must be made vacuum tight is much larger for the diffusion plant than is the case for the centrifuge plant. No pretreatment with fluorine is required for the centrifuge plant, and the control of the 33-stage centrifuge cascade is an order of magnitude easier.

Perhaps the best estimate of our unknowns in the two cases, is indicated by the relative staff which one can estimate would be required to do the necessary testing and research work. There are perhaps 1,000 people having technical ability, that is, exclusive of secretarial staff, purchasing staff, etc., on the diffusion work here, at the Carbide & Carbon Chemicals Corporation, Bell Telephone Laboratories, the Kellex Corporation, and elsewhere. Mr. Murphree and I estimate that 50 people would be able to do all that needs to be done from the standpoint of research and development, on the centrifuge plant. All the research that is being done on the diffusion plant deals with doubtful points in regard to this plant: the development of a barrier, the stabilization of the barrier, the large-scale production of barrier, the testing of seals, operation of pumps, coating steel with nickel, stabilizing the plant, etc., etc.

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In spite of the outwardly very advanced state of the diffusion plant, I cannot help but think that very serious consideration should be given to the centrifuge plant because of the far greater certainty of its successful operation, and the greater certainty of any time schedule that can be drawn up for it compared to that of the diffusion plant.

A committee should be formed to study the question of the utilization of the centrifuge method. Among the topics on their agenda should be the possibility of using presently constructed facilities for the centrifuge plant. The power plant can certainly be used with frequency changers to change the frequency from 60-cycle to 480-cycle. Perhaps the Government built Chrysler and Allis-Chalmers plants might be used to make centrifuge bowls or housings instead of diffuser cases. The buildings on site might be useable.

The committee could also consider the intermediate proposal of ordering the centrifuges for the top fifth of the plant. They could then be substituted for the rest of the diffusion plant at a moment's notice by expanding the production. Of course, in this scheme economies which are still possible by immediate substitution would not be realized.

When I consider the remarkable progress which the centrifuge project has made with a handful of men, and compare this with the enormous effort which we have expended on the diffusion plant without seeing a solution to our principal problems, the centrifuge method looks extremely attractive to me, and I feel sure that a committee which will make a serious study of the situation will agree with me.

Very sincerely,

Harold C. Urey
Director of Research

#1 Brig.Gen.L.R.Groves
#2 Dr. R. Tolman
#3 Dr. J. B. Conant
#4 Dr. A. H. Compton
#5 Dr. L. J. Briggs
#6 Dr. E. O. Lawrence
#7 Mr. E. V. Murphree
#8 Dr. H. S. Taylor
#9 Major B.K. Hough, Jr.
#10 H. C. Urey file

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COMPARISON OF PLANTS TO PRODUCE 36% X AT 1 kg/day.

STRIPPING DOWN TO 50% OF FEED

Long-Bowl Centrifuge

Diffusion Plant

* Page 2

* Page 1, cover sheet, has not been included.

December 11, 1943

Dr. James B. Conant
1530 P Street NW
Washington, D.C.

Dear Jim:

By this time you undoubtedly have received a copy of my letter to General Groves, outlining my point of view in regard to the centrifuge process. I think there is little that I can add to this, except that I wish to emphasize a point of view about some of these things which I think perhaps some of us have overlooked. I think it is possible, in situations such as this, to find from time to time a really superior process. I myself through the last years, have hoped that I might think of or find some method that would be outstandingly better than any of the other methods being considered. So far I have found nothing of the kind, nor has anyone else, unless it might be that the evaporative process that Brewer has been following, might be a partial solution of this kind.

The centrifuge process is not outstandingly better than the diffusion process; in fact, perhaps the diffusion process eventually can be made a better process than the centrifuge process. However, it does seem to me that at the present stage of development the centrifuge process is far better developed than the diffusion process from the technical side, and that the expansion from the present stage of development to a full scale plant can be made with much greater confidence for the centrifuge than for the diffusion plant, and that any time schedules for the two will be much more certain for the centrifuge process. This is because it is easier to predict times required for mechanical construction rather than times for chemical development and construction.

We should not make the mistake of thinking that just because a small group of men have worked on this up to the present time, that therefore it is necessarily less developed than the diffusion process, on which a large number of men have been engaged for a long time. May I urge speed in

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considering this problem by any committee or committees that may investigate it. This is one way that time can be saved.

Very sincerely,

Harold C. Urey
Director of Research

#1 Dr. J. B. Conant
#2 Brig. General L. R. Groves
✓#3 Dr. R. C. Tolman
#4 Dr. A. H. Compton
#5 Dr. L. J. Briggs
#6 Dr. E. O. Lawrence
#7 Mr. E. V. Murphree
#8 Dr. H. S. Taylor
#9 Major B. K. Hough, Jr.
#10 H. C. Urey file

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18 December 1943

Dr. James B. Conant,
Chairman, S-1 Committee,
1539 P Street, NW,
Washington, D. C.

Dear Dr. Conant:

As you know I have recently received a number of letters from Dr. Urey in which he expressed his views as to the value of the centrifuge method. In view of the facts that:

a. The S-1 Committee has never recommended that this method be carried forward;

b. It would appear to be impossible to complete the engineering and construction in time to be of value in this war;

c. We have already embarked on more than one method of producing the essential material;

I am doubtful of the soundness of his conclusions. I would appreciate it if you could give me your views on the feasibility from a scientific standpoint of this method.

Sincerely,

L. R. GROVES,
Brigadier General, C. E.

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OFFICE FOR EMERGENCY MANAGEMENT
NATIONAL DEFENSE RESEARCH COMMITTEE
OF THE
OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT
1530 P STREET NW.
WASHINGTON, D. C.

December 20, 1943

[redacted]

To: Brig. Gen. L. R. Groves, Deputy Chief, Construction Division, Corps of Engineers

From: R. C. Tolman

Subject: Visit to Centrifugal Plant at [redacted]ayway, N. J.

1) On December [redacted], 1943, Dr. H. C. Ur[redacted] wrote to you urging that serious consideration be given to the possibilities of the centrifugal method of separating gases. [redacted] regards the development of this process as far ahead of the development of the diffusion process. He suggests that a committee should be formed to study the utilization of the centrifugal method, including the possibility of diverting to this process facilities which have been planned or constructed for the diffusion process.

2) In accordance with our discussion of this letter, I visited, on December 15, the pilot centrifugal plant which is being developed and operated by the Standard Oil Development Company at their laboratories at Bayway, New Jersey. I was able to see the plant in operation, and to discuss its design and performance with Mr. E. V. Murphree, Vice President, Mr. Paul H. Kuhl, Associate Manager of the Process Division, Mr. W. I. Thompson and others involved.

3) The pilot plant consists at present of a single centrifugal unit, with an extruded duralumin bowl 38 inches long by 7.34 inches internal diameter rotating at about 470 r.p.s., together with space and auxiliaries, except for piping, sufficient for the erection of a 24 unit plant. The principal auxiliaries consist of (a) motor generator sets for providing the electrical power needed for starting and operating the units, (b) facilities for furnishing the centrifuge units with lubricating oil, tempered water, nitrogen seal gas, and hydrogen casing gas, (c) feed bath, flowmeters, pressure recorders and intermittent type trays for handling process gas, and (d) an ammonia-ethylene refrigeration system for chilling traps and

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drying the nitrogen seal gas.

4) The centrifugal units have been designed and fabricated by the Westinghouse Company. At the time of my visit, after preliminary experimentation with a bowl fabricated from a duralumin forging, the Standard Oil Development Company had carried out experimental operation with two full strength extruded bowls, the second one with a corrosion proof monel shaft and an improved damper system.

5) The total period of mechanical operations of these two machines up to the time of my visit was 93 days, with four shut downs. Only one of these shut downs was due to failure in a rotating part of the units. This instance, involving seizure of an oil flinger attached to the lower centrifugal pump, was described as probably due to improper assemblage. Two shut downs were due to air leakage at a hand hole cover and at a faulty valve, and the remaining shut down was made to install the second bowl. At the time of my visit the unit was still in continuous operation after 31 days without shut down.

6) The total period of operation of the two machines with process gas at the time of my visit was 67 days. Figures available for the first 58 days of this operation, show that about 600 lbs. of feed material had been processed with the production of about 400 lbs. of "waste" material with a depletion of the lighter component by roughly 1%, and the production of about 100 lbs. of "product" with an average enrichment of about 4%. The plant is operated on a three-shift continuous basis, with three non-technical operators per shift. This type of operation involved cascade-current flow, with removal of product and addition of make-up in such a way as to simulate conditions in a complete production plant.

7) The company has carried out a number of runs on the plant to determine thru separative work performed as a function of flow rate and of product withdrawal rate, and is now carrying out an endurance test at one particular flow rate with product rates near the optimum for that flow. The separative work at optimum product rate for a given flow rate is of the order of 74 to 76% of that which would be theoretically predicted neglecting mixing. The company has also carried out laboratory work on the collection of process gas in cooled traps, on corrosion, on the development of small special valves, and other laboratory problems, and is working on the development of a trapping mechanism for the continuous removal of process gas from nitrogen containing streams.

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8) Further work at the pilot plant stage would involve the development and testing of the 12S inch bowls proposed for production plant operation, completion of the development of traps for the continuous removal of process gas from the nitrogen seal gas, and possibly the construction and operation of a 24 unit plant for testing methods of plant control. Some work is continuing at Westinghouse, at their own initiative, on the development of the 12S inch bowl, and results at Virginia indicate that a 12S inch bowl should be practicable. The proper development of such a bowl would in any case, however, take several months. No firm arrangement has yet been made for the future continuation of work either by Westinghouse or by the Standard Oil Development Company.

9) A comparison of the present stages of pilot plant operation for the centrifugal and diffusion processes will be found at the bottom of the table, accompanying Dr. Urey's letter of December 7. It will be noted that the centrifugal pilot plant has been operated somewhat longer than the diffusion pilot plant, has about 220 times as great a capacity for doing separative work, and is very much more nearly similar to what would be used in a final production plant.

10) A comparison of certain general features of production plants, using the two alternative methods to produce 1 kilogram of X per day at 30% concentration, will also be found in Dr. Urey's table as well as in the body of his letter. There are a number of important differences which may be specially mentioned. The centrifugal plant would have 29,000 machines as compared with 3,520 for the diffusion plant, each machine handling less gas but producing more separation. This means many more pipe connections for the centrifugal plant and a considerably greater length of process pipe but of much smaller size. The total area exposed to process gas would be much smaller in the centrifugal than in the diffusion plant, leading to a calculated rate of consumption of process gas of 230 grams per day as against 3,800 for the diffusion plant. The operation of the centrifugal plant would not involve pre-treatment with fluorine, and there would be nothing analogous to barrier plugging which would change the operating conditions with time. The auxiliary services to supply lubricating oil, tempered water, dry nitrogen seal gas, and hydrogen casing gas to the centrifugal machines might perhaps be somewhat more elaborate than the similar auxiliary services for the diffusion machines. The cost and power consumption for the two plants would be of the same order of magnitude.

11) Any comparison of the unsolved problems for the two processes is of course somewhat subject to doubt. The principal unsolved problem

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for the centrifugal process would seem to be the development of the 126 inch bowl. This would appear to be a feasible job, but would certainly take some months. The principal unsolved problem for the diffusion process would seem to be the perfection of a suitable barrier, fabricated in tubular form. This would appear to be a job still involving much research, but one which is now being attacked vigorously by many competent investigators, with increasing prospects of success, and probably soluble within six months to a year. For both processes the matter of developing a practical engineering vacuum technique for the large plants involved is a difficult one. In this connection, the centrifugal process has the advantages arising from the smaller total volume and smaller sizes of connecting piping, but the disadvantages arising from the very much larger number of units that have to be connected. The operating problems for both processes are still a matter for study, although more attention has presumably been given to this matter for the diffusion than for the centrifugal process. In either process the failure of a unit would presumably involve the immediate isolation of that unit and perhaps of neighboring ones from stream. If the probability of failure per unit were the same for the two processes, the number of failures would be much greater for the centrifugal than for the diffusion plant. There are probably greater possibilities for the sudden failure of a centrifugal unit which would wreck a portion of the plant than for such a failure of a diffusion unit. It is proper to note, as has been emphasized by Dr. Urey, that unsolved problems for the diffusion process tend to be of a physical chemical character, while those for the centrifugal process tend to be of a mechanical character, and hence to have solutions for which it is easier to predict the character and time schedule.

12) A comparison of the status of production plant design for the two processes is of course favorable to the diffusion process, since this has not been authorized or undertaken for the centrifugal process. By making the bold - and to be hoped for valid - assumption that a suitable tubular barrier will be forthcoming, it has been possible to go ahead with the detailed design of the diffusion plant and this is well advanced, including layouts for buildings, power plant, auxiliary facilities, and the diffusion units themselves. By making a similar assumption that the 126 inch bowl will be procurable, it would now be possible to make nearly as detailed a layout for a centrifugal plant, but this has not been done.

13) A comparison of the status of construction and procurement for the two kinds of plant is of course also favorable to the diffusion

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process. Here again the assumption that suitable tubular barriers would ultimately be obtainable, has made it possible to start construction and to let contracts for procurement for nearly everything except the barrier tubes themselves. A similar advance construction and procurement program has not been authorized or undertaken for the centrifugal process, and would have to be less complete than for the diffusion process, since the long bowl machines, which have not yet been designed, would form such a large fraction of the total necessary procurement. Facilities for the extrusion and machining of duralumin could, however, be set up in advance, which would be important in view of the tremendous magnitude of the problem of fabricating some 50,000 machines, the success of which would be dependent on extreme care and accuracy in manufacture.

(4) The foregoing comparisons between the two processes are of course incomplete and probably in some ways not entirely correct. It is understood, however, that Dr. Conant has requested Drs. Murphree to present his views as to further work that should be done on the centrifugal process, and as to its future possibilities. Dr. Murphree said that his report on this would be ready at the end of the present week. On the basis of this report, broader comparisons will be possible. In the meantime it might be desirable to assure Dr. Urey that the matter is not being neglected.

Sincerely yours,

Richard C. Tolman
Vice-Chairman
NDRC

RCT:mll

cc: Dr. Conant

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OFFICE FOR EMERGENCY MANAGEMENT

OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT

1530 P STREET NW.

WASHINGTON, D. C.

VANNEVAR BUSH
Director

26 Broadway
New York 4, New York
December 22, 1943

Dr. J. B. Conant
1530 P Street, NW
Washington 25, D. C.

Dear Dr. Conant:

In accordance with your letter of December 9, we have prepared a rough time schedule for large centrifuge plants. Two sizes of plants have been considered; one to produce 1 kg./day of light material of 90% concentration and the other produce half this amount. In both cases it has been assumed that the light material recovered would be 50% of that present in the initial feed. The number of centrifuges required is roughly 30,000 in the first case and 15,000 in the second case. These are long bowl centrifuges. It has been assumed that a good priority corresponding to that being obtained on similar projects will be applied to this job.

The major equipment involved, which is considered critical from a time standpoint, are the centrifuges themselves, the electrical equipment required to drive the centrifuges, and seal gas recovery equipment. The Westinghouse Electric Elevator Company has made a fairly careful study of the production of the long bowl centrifuges, so the figures given for the possible production dates of these centrifuges is in no sense a guess. The Westinghouse Electric & Manufacturing Company has investigated the production of the electrical equipment for driving the centrifuges and the figures given are reasonably reliable. The manufacture of seal gas recovery equipment has been developed with Clark Brothers who have fabricated experimental equipment for us. Here again we believe the figure given as to time is reasonably accurate. A discussion of these various pieces of equipment follows:

Centrifuges

The Westinghouse Electric Elevator Company have prepared a time schedule for centrifuges based on installing facilities that will ultimately produce 1,000 centrifuges a month. On the basis of this schedule their estimate calls for production of 600 centrifuges in 13 months after word is given to go ahead. Most of this time is, of course, spent in the development and erection of manufacturing facilities. After this 13-month period

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centrifuges would be produced at the rate of 1,000 per month. The investment for centrifuge manufacturing facilities on this scale would be roughly $3,000,000 and the manpower required in their fabrication would be about 1,500. The estimated cost of production of the centrifuges without any payout on the fabrication plant is $2,100 per machine. This includes the electric motor for driving it. It is pictured that the centrifuge fabrication facilities, except for work that can be sub-contracted, would be erected with government money. It would undoubtedly be desirable to install the centrifuge assembly plant, included in the above investment, at the centrifuge plant site so that this equipment can be used for maintenance of the machines. The scale of production of centrifuges can be multiplied with a corresponding increase in rate of production. That is, through installation of two of the units contemplated by Westinghouse the production of centrifuges in the first 13 months' period could be increased to 1,600 machines, and rate of production thereafter 2,000 machines per month. Figures given on the time schedule for centrifuge production assume the successful completion of the development work on long bowl centrifuges by the Wesginthouse Research Laboratory in six months. This is discussed in a separate letter.

Electrical Equipment

Rough plant layouts indicate it may be desirable to group the centrifuges in individual buildings, each building containing about 3,000 centrifuges. For a 3,000 centrifuge group, 8 motor generator sets will be required of 3,000 KVA capacity to convert 60 cycle current to 480 cycle current. For a 1 kg./day plant 80 such motor generator sets will be required. Westinghouse estimates that the first unit can be delivered in thirty weeks from date of order, and that one additional machine can be delivered each week thereafter. The total program would require 26 months for the 1 kg./day plant. It seems likely that larger units of 4,000 KVA can be supplied, which would reduce the total number of units for a large plant to 60. On this basis deliveries of the motor generator sets would be completed in 21 months. In either case, the motor generator sets will be available ahead of the centrifuges they are to serve. Other electrical equipment such as accelerating motor generators for starting the centrifuges, transformers, switches, and wiring can be obtained well ahead of the centrifuges for which they would be required. Electrical equipment is therefore not limiting.

Seal Gas Recovery Equipment

The gas centrifuge as now developed is not based on maintaining particularly tight seals between process gas and nitrogen used for purging purposes. As a result there is a considerable amount of seal gas coming off each machine which is a mixture of process gas and nitrogen. It is necessary to recover the process gas from this stream and return it to the centrifuge stage from which it comes. In the pilot plant at Bayway cold traps which are operated on a batch basis are used for this recovery. A continuous cold trap has been built and has been recently operated experimentally. This continuous

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trap seems to function all right although its capacity has not been determined. It is estimated that for a 1 kg./day plant 600 continuous traps of the size now being tested at Bayway will be required. It is not felt that difficulty will be experienced in getting these traps in adequate time to meet the centrifuge schedule.

Remainder of Equipment

The remainder of the equipment will consist of buildings, vacuum pumps, piping and miscellaneous equipment. It is believed that all this material can be fabricated and erected to meet the schedule on the centrifuges. It is therefore felt that the limiting item determining plant completion date is the fabrication of the centrifuges.

TIME SCHEDULE

As brought out above, in computing a time schedule two sizes of plants are being considered; one to produce 1/2 kg./day of light material in 90% concentration, and the other to produce 1 kg./day. For the smaller case the time schedule has been based on centrifuge fabrication facilities which, in full operation, would produce centrifuges at the rate of 1,000 per month. For the large plant the centrifuge fabrication units have been doubled, so that when in full operation they would produce centrifuges at the rate of 2,000 machines per month. In considering the time schedule it would seem desirable for either the small or the large plant to erect the plant as five separate units. On this basis the individual units for the smaller case would have a capacity of 100 grms/day of light material, and for the larger case 200 grams/day of light material. 15,000 centrifuges would be required for the smaller case, and 30,000 for the larger. The individual units for the smaller case would consist of 3,000 machines, and for the larger case 6,000 machines. This arrangement has the advantage that any troubles experienced in one unit would not be reflected in the other units. The equilibrium time for the individual units would be four months; that is, product could be taken off at the estimated design rate four months after the unit was put in complete operation. By building the plant as individual units, product would be obtained much earlier than would be the case if the plants were built as one complete unit.

Time schedules have been based on the assumption that decision to go ahead with the project would be made January 1, 1944. The schedule can be adjusted for a later decision date. On the basis of this date, it is estimated that construction of one unit for either the large or small plant would be completed April 15, 1945, and that the plant would be in full operation May 15, 1945, and would be producing product at design rate September 15, 1945. At this date, depending on whether the large or small plant was built, the production rate would either be 100 grams or 200 grams per day of light material in

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90 concentration. Additional units of corresponding capacity would start producing product in accordance with the following schedule:

2nd Unit	-	December 15, 1945
3rd Unit	-	March 15, 1946
4th Unit	-	June 15, 1946
5th and final Unit	-	September 15, 1946

COST OF PLANT

In connection with the time studies that have been made, cost of certain equipment has been developed. These costs are as follows and are based on a 1 kg./day unit.

Centrifuge fabrication facilities	$ 6,000,000
30,000 Centrifuges @ $2,100 each	63,000,000
Electrical equipment including wire and installation	21,000,000

For a half kg./day plant these figures would be roughly cut in half. An estimate of the cost of the complete plant has not been made in the present study due to the fact that this really has to be developed in detail.

In order to get a rough picture of the possible total plant cost an estimate may be used which was prepared some time ago by The Kellex Corporation for centrifuges to take product of roughly 36% concentration from the diffusion plant up to 90% concentration. This plant involved 624 long bowl centrifuges and the cost of the plant without offsite facilities was estimated at $4,372,000. Pro-rating this up to a 30,000 centrifuge plant on a direct basis would give an investment of the order of $200,000,000. For offsite facilities such as cooling water, steam, shops, laboratories and the like, but not including electric power generation, it is suggested the above figure be increased by about 30%, which would give an overall investment of $260,000,000. For a half kg./day plant this would be roughly cut in half. It should be appreciated that this figure represents a very large extrapolation of the estimate prepared by the Kellex Corporation and it may be that the actual cost, depending on the original accuracy of the estimate, would be somewhat lower due to the larger scale.

You will appreciate that the time schedule as well as the production from the plant will depend on the successful completion of an experimental program on the centrifuge process, which is covered in a separate letter of the same date. In order to meet the time schedules outlined it will be necessary to carry out the development program along with the plant construction program. This is, of course, taking the same sort of a chance on the project as is being taken on other projects of similar type, where, if unforseen difficulties are experienced considerable money may be wasted.

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I will be glad to attempt to supply any further information you may wish on the time schedule for a large plant.

Very truly yours,

E. V. MURPHREE

EVM:BF
Copy to Dr. L. J. Briggs
Dr. A. H. Compton
Dr. E. O. Lawrence
Dr. H. C. Urey
Brig. Gen. Leslie R. Groves

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OFFICE FOR EMERGENCY MANAGEMENT

OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT

1530 P STREET NW.

WASHINGTON, D. C.

VANNEVAR BUSH
Director

26 Broadway
New York 4, N.Y.
December 22, 1943

Dr. J. B. Conant
1530 P Street, NW
Washington 25, D. C.

Dear Dr. Conant:

I am writing to outline an experimental program which, in our opinion, should be undertaken if serious consideration is to be given to further development of the centrifuge project. Before going into the program I would like to outline the present status of the centrifuge process.

Short Bowl Centrifuges

The University of Virginia has operated a short bowl centrifuge with process gas to obtain enrichment data. This particular centrifuge was not developed mechanically to the point where it could be used for large plant operation. Enrichment data, however, should be reasonably reliable. The results obtained at the University of Virginia indicate a cascade efficiency, as compared with a theoretically perfect centrifuge operating under their conditions, of about 60%. The Research Laboratory of the Westinghouse Electric and Manufacturing Company has constructed short bowl centrifuges of a type which are believed mechanically suitable for large plant operation and these machines have been operated by the Standard Oil Development Company to obtain data on enrichment with process gas. A review of the results obtained by the Standard Oil Development Company was transmitted to the committee by my letter of December 6, 1943. On the whole the operating experience has been quite satisfactory. Mechanical operation in various runs has been carried out for a total of 101 days, of which 75 days have been on process gas. The last run, which is still going on, has now covered a total uninterrupted operating period of 39 days, of which 38 days have been on process gas. Enrichments obtained in the Standard Oil Development Company pilot plant have been somwhat better than obtained at the University of Virginia, probably due to better control of operating variables. A cascade efficiency, based on a theoretical centrifuge operating under the same conditions, of around 75-80% has been obtained. On the basis of this efficiency, extrapolated to a long bowl centrifuge, about 30,000 such machines would be required for a plant to produce 1 kg./day of light material in 90% purity and to recover 50% of the light material in the feed. In connection with cascade

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efficiency, it should be borne in mind that the theory does not allow for radial flow in the centrifuge whereas actually, due to pressure drop required for flow of gas through the centrifuge, it is likely some radial flow does occur. If the theory allowed for this fact the predicted results would probably come quite close to the actual.

Long Bowl Centrifuge

The short bowl centrifuge has a length of about 36 inches whereas the length of the long bowl centrifuge is about 132 inches. The primary difference between the short and long bowl centrifuge is that in the short bowl machine the bowl itself is not subject to critical vibrations, whereas in the long bowl centrifuge it is. In going from a short bowl to a long bowl centrifuge it is therefore necessary to make additional provision for damping to take up the energy involved in the vibration of the bowl as it goes through its critical speeds. The gas pumps, gas seals, and the like are identical for the long and short bowl machines. The development work in going to the long bowl machine is therefore primarily concerned with the damping problem, and advantage can be taken of all the developments that have been made in the short bowl machine in regard to other features.

The University of Virginia has built and operated a long bowl machine of the diameter contemplated for large plant use. In the large plant it is planned to operate the centrifuge at 472 RPS. The bowl the University of Virginia has is a forged bowl rather than an extruded bowl, and as a consequence it is not considered safe to operate it at the full speed of 472 RPS. In the particular machine at the University of Virginia they have been up to a speed of 430 RPS but this is quite close to a critical vibration speed that starts at 375 RPS and as a consequence represents an undesirable operating point. From the strength of the bowl it was not feasible to go well above the critical speed and to keep safely below it the machine has been operated at 350 RPS. Under these conditions the long bowl centrifuge has given a cascade efficiency of around 75% of that predicted for a theoretical centrifuge operating in the same way. The results on the long bowl centrifuge at the University of Virginia therefore check quite well with the results on the short bowl centrifuge at Bayway. There is no indication that there will be a difference in separation work per foot of length in going from a short bowl to a long bowl machine.

The Research Laboratory of the Westinghouse Electric and Manufacturing Company had a contract for development of a long bowl centrifuge which was terminated after the September meeting of the committee at Knoxville. At that time considerable progress had been made in studying the various factors involved in the long bowl centrifuge, and particularly Westinghouse felt they had worked out the damping problem. After the work was terminated Westinghouse went ahead at their

own expense and carried the development further of building a long bowl centrifuge which contained all of the features necessary for studying the dynamics of this machine. This machine would operate normally at a speed of about 472 RPS. In getting up this speed the machine goes through four critical speeds; the first being at about 17 RPS, the second about 33 RPS, the third about 100 RPS, and the fourth and last at about 300 RPS. The machine went up successfully through all these critical speeds and operated quite smoothly at 348 RPS. Difficulty would not be expected in going up to the operating speed of 472 RPS due to the fact that no further criticals are encountered. The machine put together was considerably out of balance and no attempt was made to balance it due to the fact that Westinghouse wished to obtain experience on how it would operate in unbalanced condition. On bringing the machine down there was a failure in going through the 300 RPS critical. The shaft pulled away from the bowl, probably because the fastening was not strong enough and the machine was wrecked. It is Westinghouse's feeling now that better balancing would be required for a machine and they are quite confident that they can develop a machine that will give satisfactory mechanical operation.

Recovery of Seal Gas

In both the short and long bowl centrifuges it is planned to use dry nitrogen seal gas to keep process gas away from oil. There is a fair amount of flow of gas through the seals and one of the sealing gas streams from a machine contains a mixture of nitrogen and process gas. It is necessary to recover the process gas from this stream and return it to the same stage in the cascade from which it comes. While it may be possible to make this recovery with normal cold traps, which are operated on a batch basis, it is felt desirable to use continuous traps and we have developed such a trap designed to give very low holdup and designed so that all cooling surfaces are scraped so that adequate heat transfer can be obtained. A model trap has been built by Clark Brothers and has recently been operated at Bayway. After some initial difficulties the trap now gives quite satisfactory operation but its capacity has not been determined. It is felt that further development work will be necessary on recovery of process gas from the sealing stream, although it is felt from what we know now that the continuous trap will offer a satisfactory solution.

Proposed Experimental Program

From the above background the following program is proposed on the basis that it is desirable to push the centrifuge project as rapidly as is consistent with getting reliable information. The various parts of this proposed program are as follows:

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(1) Development of Long Bowl Centrifuges $80,000

The Research Laboratory of the Westinghouse Electric & Manufacturing Company estimates that six months will be required to complete the development of a long bowl centrifuge, and that the cost of this development will be $60,000. In addition to this there are certain other studies connected with bearings and development of a single motor for the long bowl centrifuge that are estimated to cost $20,000, making a total of $80,000. It is felt very strongly that this work should go ahead if the centrifuge project is to be carried any further than at present.

(2) Construction of Long Bowl Centrifuges for Bayway Pilot Plant 50,000

Based on the development under Item (1) the Westinghouse Electric Elevator Company would construct two long bowl centrifuges for operation at the Bayway pilot plant. The Westinghouse Electric Elevator Company estimates that the first machine can be completed five months after they receive material, and the second machine six months after receipt of material. If prompt action is taken it may be possible to obtain material in three months, which would mean that the first machine would be available in eight months and the second in nine months.

(3) Operation of Long Bowl Centrifuge at the University of Virginia 50,000

It will be desirable to get further information on the enrichment that will be obtained under varying conditions with the long bowl centrifuge at the University of Virginia. It may also be desirable to provide the University of Virginia with an extruded bowl which can be operated at design speed. The amount allowed for would cover six months' operation in 1944.

(4) 9 Additional Short Bowl Centrifuges 100,000

If the centrifuge project is to be pushed it is felt desirable to build a cascade of 9 short bowl centrifuges to obtain experience on cascade operation. The centrifuges as

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designed are self-metering as to process gas and the operation of the cascade will determine how reproducible the flow of process gas through an individual machine will be. It is contemplated that the cascade will be operated with continuous traps and such an operation will allow experience on this feature. It is estimated that the 9 short bowl centrifuges can be produced by Westinghouse Electric & Manufacturing Company in nine months after receipt of order.

(5) Installation of Long Bowl and Additional Short Bowl Centrifuges $100,000

This item covers the installation of the long bowl centrifuges and the cascade of 9 short bowl centrifuges at the Bayway pilot plant. On the basis of starting January 1, 1944 it is expected that operation of the pilot plant on both the long bowl centrifuges and on the cascade could start October 15, 1944.

(6) Operation of Bayway Pilot Plant. 315,000

The operating program for the Bayway pilot plant is based on continuing operation of a short bowl centrifuge up to the time when long bowl centrifuges and additional short bowl centrifuges for a cascade are available. This would cover a nine months' period at an estimated cost of $25,000 per month. After the short and long bowl centrifuges are available it is estimated the operating cost will be $30,000 per month, and there would be three months in 1944 at this cost, which makes the total given above. The operation of the short bowl centrifuge would provide additional experience on enrichment and mechanical reliability of this machine.

(7) Development of Seal Gas Recovery System. . . 50,000

This work would be carried out by the Standard Oil Development Company and would involve development of a demonstrated satisfactory means of recovery of process gas in the seal gas from the centrifuges.

The program as outlined above involves an expenditure of $745,000 for research and development work, along with equipment required for 1944. It is likely that additional operation

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of the pilot plant in 1945 would be desirable if a large commercial unit is to be installed, or possibly for completion of the development in any case. If it is desired to proceed purely on the development of the process without going ahead with erection of a large plant, it is possible that expenditures could be curtailed somewhat although not a great deal. It is felt that work on the long bowl centrifuge should continue in any case. It would be possible to eliminate work at the University of Virginia, involving an expenditure of $50,000, and it is possible that the operation of a cascade of short bowl centrifuges is not entirely necessary, although this is questionable. This would reduce the cost by a further $200,000. If it is desired simply to continue the operation of the short bowl centrifuge now installed at Bayway without doing anything further, this can be continued at an estimated cost of $25,000 per month. From our own standpoint we are not particularly interested in this case but we will continue the operation if O.S.R.D. desires.

In addition to the above items in the program, it may be desirable for the Aluminum Company of America to carry out some work on the strength of extrusions that will be used for bowl fabrication, particularly from the standpoint of seeing if materials of improved strength could be obtained which would allow the machines to operate at somewhat higher speeds. The necessity and cost of this program have not been clearly established.

If you have any questions in regard to any of the material given in this letter I will be glad to try to answer them.

Very truly yours,

E. V. MURPHREE

EVM:BF

Copy to Dr. L. J. Briggs
Dr. A. H. Compton
Dr. E. O. Lawrence
Dr. H. C. Urey
Brig. Gen. Leslie R. Groves

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OFFICE FOR EMERGENCY MANAGEMENT

OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT

1530 P STREET NW.

WASHINGTON, D. C.

VANNEVAR BUSH
Director

December [illegible], 1942

Brigadier General L. R. Groves
Office of the Chief of Engineers
P.O. Box 2610
Washington, D. C.

Dear General Groves:

(1) This is in reply to your letter of December 1[illegible], 1942. You ask the S-1 Committee to give their views on the feasibility from a scientific standpoint of the centrifuge method. It is not possible to convene the Committee in the short time available, for I know there are urgent reasons why we should come to a decision in this matter in the next few weeks. You have received copies of two letters from Dr. Murphree giving the present status of the research program and the estimates of the construction program. Both letters are dated December 22. These letters have likewise gone to each member of the S-1 Committee as have his progress reports dated November 2, November 10 and December 7. The members of the Committee therefore have all the pertinent facts at their disposal. I am asking each member of the Committee to write you as soon as possible his own answer to the question which you have raised. I feel that you can be guided better by these separate replies than by any formal vote which might now be taken by the Committee after discussion.

For the sake of the record, I am enclosing a copy of a portion of the last minutes of the S-1 Committee at which this matter was discussed, which will show you the opinion of the Committee at that time. As you know, however, some members of the Committee have reversed their view in the meantime because of the excellent results obtained at Rayway, which were not a surprise to me at all as I always assumed the instrument would work much the way as has been demonstrated.

(2) My own opinion is as follows. I believe that it is evident from the time schedule given in Dr. Murphree's letter of December 22 that it is impossible for the centrifuge method to be brought into the present program except as an additional insurance against the failure of the present three methods which are now being pushed. I have every reason to believe from my contact with the present status of these three methods that each one, if successful, will be in production at the rate of one kilogram a day months before the centrifuge plant could come in at the same rate. I am convinced that the United States Government

of both 25 and 49

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is now spending all the money, time and material on this program that is justified and that an additional process for insurance, particularly insurance maturing at such a late date would be entirely unjustified. I should not want to take the responsibility therefore of recommending any expenditure of money on this program, even making the most favorable assumptions as to the future research, development and construction.

By this same token, unless you and the Military Policy Committee should decide to take up this program and incorporate it in your work, I do not feel that the S-1 Committee should continue to spend any further money on research or development.

As to the feasibility of the centrifuge method from a scientific standpoint, it seems clear from the evidence presented to date that the method is a favorable one and that there is every reason to believe that the time schedule and estimates given by Dr. Murphree could be realized. Therefore, one can say there is a strong probability of success, but from my point of view no more certainty than is now presented by the electromagnetic, the graphite pile or the diffusion process.

Very sincerely yours

James B. Conant

James B. Conant

cc: Dr. L. J. Briggs
Dr. A. H. Compton
Dr. E. O. Lawrence
Dr. E. V. Murphree
Dr. H. C. Urey
Dr. Irvin Stewart

[redacted]

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Extract of Minutes of Meeting of S-1 Committee Held September 10-11, 1943

After discussion it was voted to permit the Virginia contract to run to its completion date, i.e., not to stop work there at present and at least to have Standard Oil continue to spin the short bowl for an aggregate of 4 months, at as small an expense as possible.

Dr. Murphree pointed out that Westinghouse had requested an additional $65,000 to cover their work on centrifuges. Of this amount $25,000 was for completion of the short bowl contract and $40,000 for completion of the long bowl contract. The complete work for the balance of this year on the centrifuge pilot plant, involving operation of a short bowl centrifuge on process gas and to cover certain miscellaneous services (such as a certain amount of work on the Trail project), Standard will need an additional $125,000. To operate the long bowl centrifuge for the first six months of 1944 Standard estimates that $120,000 will be involved. Work at the University of Virginia may be completed this year, although it is possible an added expenditure of $25,000 for next year may be desirable. Summarizing, Dr. Murphree's recommendations are as follows:

Westinghouse	$ 65,000
Standard Oil	125,000
Standard Oil	120,000
Virginia	25,000
	$335,000

On a motion to authorize the above complete program, the vote was a tie, and the decision is to be made by Dr. Bush without benefit of any recommendation from the Committee (Lawrence, Compton, and Murphree in favor of the full program; Conant, Briggs, and Urey in favor of the curtailed program previously voted).

[redacted]

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OFFICE FOR EMERGENCY MANAGEMENT
OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT
1530 P STREET NW.
WASHINGTON, D. C.

VANNEVAR BUSH
Director

26 Broadway
New York 4, New York
December 31, 1943

Brig. Gen. Leslie R. Groves
Box 2610
Washington 25, D. C.

Dear General Groves:

In accordance with Dr. Conant's letter of December 28 I am writing to give my views relative to what action should be taken on the centrifuge project. On this project I have given consideration to three methods of procedure:

(1) To proceed as rapidly as possible with the construction of a large plant to produce 0.5 to 1 kg./day of light material.

(2) To finish the development work to the point where construction of a plant can be started at any time but with the general thought that no attempt would be made to apply the method in this war.

(3) Drop the whole project.

In regard to Items (2) and (3), which can be disposed of easiest, an experimental program was outlined in my letter to Dr. Conant of December 22. This program called for an expenditure of $745,000 for the year 1944, and there might be further expenditures due to experimental work running into 1945. I think the best way to look at this would be an expenditure of $1,000,000 to complete the project from the standpoint of development work required. It might be possible to scale down this program somewhat by eliminating the installation of a small cascade of short bowl machines. In this case the expenditures for 1944 would be roughly $600,000 and further work required in 1945 might add $200,000 to give a total of, say, $800,000. I have some question as to the wisdom of continuing development work if the process is not to be used in this war, although I doubt if I have sufficient background to pass on this question.

The Standard Oil Development Company would only want to continue the development work if it was felt that a real service would be rendered to the government, otherwise it would be preferable to put the men involved on other work that is of importance to the war.

The desirability of proceeding now with construction work on a large centrifuge plant is made largely on the status and prospects of alternative means of accomplishing the same ends. At the present time work is going ahead rapidly on the electromagnetic and diffusion projects. In general, it would seem that two processes for carrying out the separation involved (which is strictly a physical separation) is about all that provision should be made for. The question therefore is whether the centrifuge project would offer a better means of carrying out the separation than the two processes now being worked on. An important factor is whether replacement of one of the existing two methods by the centrifuge method would result in serious loss in time before production could be started. From what background I have, which is not too recent, the electromagnetic project is too far along to warrant consideration being given to its replacement. The question, I think, therefore, is whether it is desirable to replace the diffusion project with the centrifuge project. If this were done, presumably the engineering work could be carried out by Kellex and the operation by Carbide, as is now planned for the diffusion project.

I do not feel that I have enough information on the present status of the diffusion project to make any intelligent recommendation as to whether it ought to be replaced by the centrifuge project. From what information I have, I believe that the centrifuge project has given a better demonstration of the feasibility of carrying out the separation than has so far been obtained on the diffusion project. I further understand that considerable difficulty is being experienced with certain phases of the diffusion project. I do not know how serious these difficulties are. From the standpoint of time, I understand it is now estimated that a large diffusion plant will be ready to operate July 1, 1945. This plant presumably would have capacity to produce 1 kg./day of light material in about 36% concentration. I further understand that enrichment above this may be obtained by the electromagnetic process. On the basis of the plant being ready to start July 1, 1945, I would think the earliest date that production would be obtained would be four months thereafter, which would be November 1, 1945. On the time schedule which I gave to Dr. Conant in my letter of December 22, which admittedly is rough, the centrifuge plant would be producing at the rate of about 0.4 kg./day in 90% concentration by December 15, 1945. On paper the diffusion project would be producing at a higher rate by the first of 1946 than the centrifuge project but the centrifuge project would start production sooner. I think there is some question if replacement of the diffusion project by the centrifuge project will cause any great delay in the production of material. This, of course, depends on how successful the development work is on the two projects.

I think it will be very difficult for the S-1 Committee to really give a sound recommendation as to what ought to be done unless a very thorough review is made of the present status of the diffusion and centrifuge projects. I believe it would be

[redacted]

[redacted]

preferable for you to appoint a committee for this, the committee to be composed of people who are familiar enough with the two projects to go into them in some detail. This committee could then give what should be sound recommendations as to what should be done. I think the question is of sufficient importance from a national standpoint to warrant such a committee investigation.

Very truly yours,

E. V. MURPHREE

EVM:BF
Copy to Dr. L. J. Briggs
Dr. A. H. Compton
Dr. J. B. Conant
Dr. W. O. Lawrence
Dr. H. C. Urey

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THIS IS COPY 1 OF 6.
Series A

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UNIVERSITY OF CALIFORNIA

RADIATION LABORATORY
BERKELEY, CALIFORNIA
Zone 4

January 1, 1944

Brigadier General L. R. Groves
War Department
Office of the Chief of Engineers
P. O. Box 2610
Washington 25, D. C.

Dear General Groves:

Dr. Conant has asked me as a member of the S-1 committee to write you my views on the centrifuge program. As you know, I have favored carrying forward the centrifuge research and development, but now it seems clear to me that the program should be terminated. My reasons for this conclusion are as follows:

1. Schedules. The time schedule outlined in Dr. Murphree's letter of December [illegible] indicates that with all the luck in the world in the extensive research and development work that still lies ahead for such a project, a centrifuge plant could not be built and put into operation soon enough. It is too far behind the graphite pile and electromagnetic programs, and in any case could not be ready for this war.

2. The Potential Ultimate Value of the Centrifuge System. At the risk of conveying an impression of prejudicial judgment, I should like to express my personal opinion, which is shared by Professor Oliphant, that the electromagnetic system is potentially more economical than the centrifuge. Two years ago, and perhaps even a year ago, it was generally felt that the electromagnetic method was the method of expediency, the costliest, but the surest and the quickest, and that ultimately it would be superseded by the much more economical centrifuge or diffusion plants. I suspect that this notion still persists, and that there is not a full appreciation in all quarters of how far the electromagnetic developments have gone. To put the matter in concrete terms, at the present time the cost estimates for the Alpha 3 electromagnetic plant are not greatly in excess of the current estimates of the centrifuge and diffusion large scale plants, and we see where further developments in the near future will increase output, resulting in cutting the overall costs of the electromagnetic plants at least to one-half, and possibly to one-quarter. The idea, therefore, that the electromagnetic system is not competitive in the long range view is untenable; rather, the reverse is true.

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3. K-25. I should like to take this occasion to set down my present views on the K-25 project, which I have indicated recently in several conversations with you; for the above remarks in regard to the centrifuge apply here with equal force. I think all will agree that a good case was made a year ago for going ahead immediately with plans for diffusion plant construction before all fundamental laboratory problems had been solved, notably the discovery of a suitable membrane. On the basis of what we were told, the optimistic hopes of the laboratory people on the one hand and the assurance on the part of Mr. Keith of the Kellogg Company that the membrane problem was practically solved, it seemed to be a good gamble to go ahead with all-out plant development. At this point I should like to repeat that I take off my hat to you for the courage and vigor with which the decision was made and has been implemented. There is no one who appreciates more than I that without your fine qualities of leadership, wisdom and courage, the overall program would not have been of any practical significance. With this respect for your judgment, I should like now to urge that, as Professor Urey has recently indicated, the K-25 program is even behind the centrifuge development, and therefore it should be immediately cut back, if not terminated altogether. I am no longer even enthusiastic about continuing research on the diffusion method, because again I feel the electromagnetic system will ultimately far outstrip it.

This is my considered opinion, although I am conscious of the fact that I may not know the whole story, but if the facts at my disposal give essentially the true picture, I feel sufficiently sure of my views to urge them to you.

Apart from general and obvious reasons for stopping a vast construction program the instant its ultimate usefulness is no longer apparent, I am immediately concerned with its effect on the program of the graphite pile and electromagnetic programs. It is clear that eliminating the diffusion and centrifuge projects would free a great many facilities which would shorten the time schedule of the former. That applies not only to construction and operation, but also to research and development. There is, for example, much that the Columbia groups could undertake immediately that would accelerate the electromagnetic project, especially in connection with the Beta process, and I know that stoppage of the K-25 program would make it possible for the manufacturers to put much more engineering talent in the debugging and development program of the electromagnetic plants in the days ahead.

I have had repeated assurances, as doubtless you have had, from all concerned with the practical aspects of the electromagnetic plants -- Stone & Webster, General Electric, Allis-Chalmers, Westinghouse and the Tennessee Eastman Corporation -- that the electromagnetic plant now under construction is operable. But it is also abundantly clear that improvements can be made which will increase the overall plant output and reduce overall costs, especially in operating personnel. The opportunity for further engineering debugging and development is great, and any amount of additional engineering and scientific talent can be immediately put to effective use in the days and months ahead.

I therefore hope very much it will be your decision to abandon the K-25 program, but in event your decision is otherwise, you may rest assured I will "play ball." I shall assume that I do not have all the facts before me and that you have good reasons for continuing.

Sincerely yours,

Ernest O. Lawrence

EOL:B

CC Dr. L. J. Briggs
Dr. A. H. Compton
Dr. E. V. Murphree
Dr. H. C. Urey
Copy Dr J. B Conant

SECRET

January 3, 1944

Dr. James B. Conant
1530 P Street NW
Washington, D.C.

Dear Jim:

I am sorry to hear of your being laid up with the grippe. I had a little siege of it myself a couple of weeks ago.

I wish to make some comments in regard to the centrifuge and K-25 problems, in reply to your letter of December 23, and am making comments on your letter to General Groves of December 28.

1. As to point of view in regard to work being done during the war on further research and development work, I am entirely in agreement with you. Unless there is some chance that the research and development will lead to results which can be used in connection with this war, the work should be carried on, if at all, in a very minor way. Applying this criterion to the K-25 leads, I think, to a rather discouraging point of view, when at the end of 1943 we still do not have a barrier for the plant. So far as I can see, we still cannot make a suitable barrier for this plant on a laboratory scale by any method. There is a chance that the K-1 will come through soon or that Dr. Mack's efforts on A barrier will be successful soon, but just as of today this is not true. It is just for this reason that I have very serious doubts of the advisability even of continuing K-25. I do not see how any time schedules can be kept when such important research and development is still going on.

On the other hand, the centrifuge project has come through with definite experiments. Successful runs have been made on a scale at least two orders of magnitude greater than have been run on K-25. I am told that 500 pounds of hex has been put through centrifuges with the compositions changed for 400 pounds by 1% downward and for 100 pounds by 4% upward, in the concentration of 25; and the centrifuge

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is steadily operating under these conditions.

2. I cannot see how you can be so optimistic about K-25, as indicated in the second paragraph of your letter. As I have watched this process since last May, devoting practically my entire time to it, I have felt that the troubles which we see in this process steadily increase, always with the hope that next week they will miraculously disappear. Specifically, there are serious problems in K-25, of a suitable barrier, of maintaining a vacuum in a very large apparatus and eliminating vacuum leaks, of a dust problem which may plug the barriers, of a control problem which is far from certain in my mind at least, of the treatment of a large plant with a very corrosive gas.

In contrast for the centrifuge, there is no barrier problem; the vacuum problem is much less, the pipes are longer but of smaller diameter, and hence leaks are much more easily corrected; there is no dust problem; there is no stabilization problem with fluorine gas; no one to my knowledge has questioned the operability of the plant from the standpoint of control problems. The deal problem seemed to have been solved in both types of plants. It seems that we will not know many of our problems on K-25 until the middle of the summer when perhaps pilot plants will be in operation.

3. In your letter to General Groves, you state that you were not surprized at the excellent results secured at Bayway, as you had always assumed the instrument would work much the way as has been demonstrated. This is a conclusion which I believe none of the men working close with it were willing to accept until the experiments demonstrated it. Two streams of gas have to flow countercurrent to each other in the bowl, and though the theory indicated that this would occur, we were not sure that disturbing influences might not completely upset them. I believe only the experiments have shown this with a clarity that made any action in regard to the centrifuge possible. I cannot understand why you think that the experimental demonstration of this thing is not an important consideration in this problem.

A year and a half ago you seemed to have concluded that the centrifuge project was out. It was also my feeling at that time that the centrifuge had little chance, but what I have observed to have happened in the last year and a half is this: the centrifuge has proved to be no worse

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than it was foreseen to be two years ago. The pilot plant with its seals has been operated and works within about 80% of theory. The plant has increased somewhat in size because of an acceptance of a lower peripheral speed with the use of aluminum and because the coefficient of diffusion is less than we estimated it to be two years ago, but otherwise nothing has changed in regard to the difficulties of the centrifuge. On the other hand, the problems of the diffusion method have steadily increased in complexity and number over that time, and hence what was a reasonable conclusion a year and a half ago, does not seem to me to be at all reasonable at the present time. Had I felt sure that the experiments now carried out at Bayway were possible, I would have pushed the centrifuge with the greatest effort I could command.

Very sincerely,

Harold C. Urey
Director of Research

#1 Dr. J. B. Conant
#2 General L. R. Groves
#3 H. C. Urey file

SECRET

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Columbia Serial No. 1[illegible]-S-475
File 11.2

COLUMBIA UNIVERSITY
DIVISION OF WAR RESEARCH
S A M LABORATORIES

January 3, 1944

REPLY TO THE UNDERSIGNED
HAVEMEYER HALL
COLUMBIA UNIVERSITY
BROADWAY AT 116TH STREET
NEW YORK 27 N Y

Brig.General L. R. Groves
P.O. Box 2610
Washington, D.C.

Dear General Groves:

In accordance with Dr. Conant's letter to members of the S-1 Committee, in which he transmits your letter to him relative to a letter which I have written to you in regard to the centrifuge method, I wish to make the following comments:

1. You asked for Dr. Conant's views on the feasibility of the centrifuge process from a scientific standpoint, and Dr. Conant has asked the members of the committee to express their opinions to you. The experiments conducted at the University of Virginia and at Bayway, show that 300,000 feet of centrifuge of the type used at these laboratories, will separate one kilogram of 25 in 30% concentration per day, and will reject uranium from which half of the 25 has been removed. We have expressed opinions in regard to various phases of this project so many times, that perhaps the impression has been given that there is never a demonstrable scientific fact in connection with the whole program. I believe the statement above is a scientific fact that has been demonstrated, and is no longer subject to opinion.

The problems of the centrifuge are engineering ones. Can 30,000 ten-foot bowls be constructed and operated? In how long a time can this be done? And at what cost and with what manpower? The Westinghouse engineers with their long experience in manufacturing high speed machinery which must run steadily and reliably should be able to answer these questions.

2. I wish to comment upon your statements (a) and (b) of your letter to Dr. Conant. The fact that the S-1 Committee has never recommended the centrifuge is not a positive proof that they should not have recommended it, or that they should not do so now. It is my opinion that our committee has never been as critical of the K-25 project, for example, at any time as it has been of the centrifuge project. I believe the reasons for this are natural ones. The difficulties of the

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centrifuge were perfectly clean-cut and definite from the very beginning. They were obvious to us all, and without definite answers on the feasibility of spinning the centrifuge and of maintaining the countercurrent flow from the centrifuge, no definite recommendation was possible up to the present time. However, the centrifuge has now been operated successfully and its problems appear to be no greater than they did two years ago. On the other hand, other methods have revealed themselves to be far more complex and difficult than they were thought to be then.

I do not recall that I have ever learned what your time limit for any of these projects may be. My own idea has been that anything not producing 25 by the autumn of 1945 is of little interest for the present war. It would seem to me that some 25 could be produced by the centrifuge plant by that time, and that the time estimates on the centrifuge plant can be made with far more confidence than can any such estimates for the diffusion plant. However, the only worthwhile opinion along this line could be made by a committee after it had studied all time schedules more carefully.

3. I can think of the following courses of action in regard to the centrifuge development:

a. The centrifuge could be used for the top of a combination diffusion-centrifuge plant. I do not subscribe to this suggestion.

b. The centrifuge plant could be run as another undertaking under Manhattan District, parallel with present projects. I would not be in favor of this.

c. The centrifuge plant could be substituted for the diffusion or electromagnetic plants. I believe I am well enough informed in regard to the electromagnetic plant to conclude that it should go forward and be completed. The possibility which I do see is the substitution of the centrifuge plant for the diffusion plant. I should like to suggest that a committee be asked to study this possibility. Also, I would suggest that the committee include in its membership the principals of all the present separation projects. Particularly, this committee should study critically, time schedules for the K-25 project as compared with such schedules for the centrifuge plant. This committee should also estimate the effect of possible difficulties ahead on meeting time schedules for both plants. Also the possibility of using the facilities both of the manufacturing and plant sites already built or designed for the K-25

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COLUMBIA UNIVERSITY, DIVISION OF WAR RESEARCH

Columbia Serial No. 1 OSRD-475

TO Brig. General L. R. Groves DATE Jan. 3, 1944 SHEET 1 FROM HCU:MMH.

~~SECRET~~ project for a centrifuge plant should be considered.

Very sincerely,

Harold C. Urey

Harold C. Urey
Director of Research

cc 1 Brig. General L. R. Groves
2 Dr. J. B. Conant
3 Dr. A. H. Compton
4 Dr. L. J. Briggs
5 Dr. E. O. Lawrence
6 Mr. E. V. Murphree
7 Dr. Irvin Stewart
8 H. C. Urey file

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U. S. DEPARTMENT OF COMMERCE

NATIONAL BUREAU OF STANDARDS

WASHINGTON

ADDRESS REPLY TO
NATIONAL BUREAU OF STANDARDS

IN YOUR REPLY
REFER TO FILE

LJB:KSV January 3, 1944.

Brig. Gen. Leslie R. Groves,
Room 5120, New War Dept. Building,
Washington 25, D. C.

Subject: Centrifuge Project.

Dear General Groves:

Dr. Conant has asked me to give you my views regarding the continuation of the centrifuge project.

The most recent reports that I have been indicate that the barrier problem in the K-25 project is far from being solved. This worries me. We have been relying on this project heavily. Let us assume that we shall still have no answer 4 months or 6 months from now. Would a substitute method then be considered? If the answer is yes, I think we should certainly proceed now with further studies of the centrifuge along the lines outlined by Dr. Smithree. If the answer is no, in other words if you think that the war will be over before a substitute method could be brought into production, then I do not see how the directive under which you are working would justify any further extension of the centrifuge project at this time.

Sincerely yours,

L. J. Briggs

Lyman J. Briggs, Director.

Copy to Dr. Conant.

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[redacted]

Metallurgical Laboratory

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January 12, 1944

Dr. James B. Conant
1530 P Street, N. W.
Washington, D. C.

[redacted]

Dear Dr. Conant:

Replying to your request for a statement of my opinion with regard to the position of the centrifuge, may I say that I would consider the experiments that have been done sufficient to establish the feasibility of this procedure as a means of preparing 25. It appears probable also that this procedure may become more efficient than either of the methods now under active construction. I am, however, doubtful with regard to the practicability of the time schedule. Will such a plant produce soon enough to be of use in the present war?

The question of the adequacy of the time schedule depends upon two factors: (1) Will other methods be producing 25 or its equivalent before a centrifuge plant can be placed in operation and (2) if the other methods are not successful in producing the material, will the centrifuge method be producing before the war has been finished following other procedures.

The latter question involves estimates of the military situation which I am unable to supply. With regard to the first question, it would now appear probable that both the K-12 and the 110 procedures should be in production before the centrifuge plant can be built. This is also true of the K-25 method, if the technical problems with which it is now faced can be overcome. Proceeding with the erection of a centrifuge plant would accordingly represent insurance against the failure of the three procedures now in hand. It will not be possible for the centrifuge method to compete with regard to time schedule with the corresponding schedules of the other three methods if these are not considerably retarded by technical or other difficulties.

It would accordingly be my judgment that in the interest of obtaining a prompt supply of 25 or its equivalent we should not undertake the task of building a centrifuge production plant but should rather concentrate our technical and scientific efforts upon the completion of the methods already in hand.

Yours very truly,

A. H. C.

Arthur H. Compton

AHC
cc: L. R. Groves
E. O. Lawrence
H. C. Urey
L. J. Briggs
R. V. Sharpless

[redacted]

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ADDRESS REPLY TO
CHIEF OF ENGINEERS U S ARMY
WASHINGTON D. C

REFER TO FILE NO _____

WAR DEPARTMENT
OFFICE OF THE CHIEF OF ENGINEERS
WASHINGTON

19 January 1942

Dr. James B. Conant,
Chairman, S-1 Committee,
1130 P Street, N. W.,
Washington, D. C.

Dear Dr. Conant:

I have now received letters from each of the members of the S-1 Committee expressing their views on the centrifuge program. The general trend of their statements make it quite clear that under our directive no further extension of the centrifuge project is justified at this time. Dr. Lawrence and Dr. Compton have excellent clear-cut resumes of the situation when they said:

(Dr. Lawrence) "The time schedule outlined in Dr. Murphree's letter of December 22 indicates that with all the luck in the world in the extensive research and development work that still lies ahead for such a project, a centrifuge plant could not be built and put into operation soon enough."

(Dr. Compton) "Proceeding with the erection of the centrifuge plant would accordingly represent insurance against the failure of the three procedures now in hand."

Sincerely,

L. J. [illegible],
Brigadier General, U. S. Army.

Copy No. 1
Dr. L. J. Briggs 2
Dr. A. H. Compton 4
Dr. E. O. Lawrence 5
Dr. J. R. Murphree 6
Dr. H. C. Urey 7
[illegible] District 8
[illegible] 9

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FOR VICTORY BUY UNITED STATES WAR BONDS AND STAMPS

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NNES Vol IV Part III
Chapter I

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SECRET

THE IONIC CENTRIFUGE

Work on an Electromagnetic Isotope Separator
Carried on at the University of California
Radiation Laboratory, January 1, 1942, to
December 31, 1942

Report

by

J. Slepian

I. Introduction

At the invitation of Dr. Lawrence, I joined the group working on electromagnetic methods of isotope separation at Berkeley, at the beginning of 1942, serving as O.S.R.D. consultant. At the same time, my assistant, Dr. W. M. Brubaker, on leave of absence from Westinghouse Research Laboratories, joined the staff at the Radiation Laboratory, and carried on experimental work under my general direction during the year 1942. Such additional assistance as was needed during the activity was always generously provided, and the group continuously engaged on this ionic centrifuge project consisted, at its maximum, of five men.

Work on this subject was continued at the Westinghouse Research Laboratories after December 31, 1942, with the further development of the basic ideas. This report, however, limits itself to the work done at Berkeley, and as far as possible, will discuss the results obtained in terms of the ideas which were held at that time.

SEC

II. General Basis for Superiority of Electromagnetic Method

Since the only feature distinguishing one isotope is its mass, and since mass, m, is defined only by the relation, ma = Force, where a is acceleration, or what comes to the same thing $\frac{1}{2}mv^2$ = kinetic energy, where v is velocity of particle, it seemed plausible that those methods of isotope separation would be most effective which give a large value to the kinetic energy of the particle. But large relative to what? Study convinced me that it was the ratio of the ordered kinetic energy of the particle to its random or thermal energy which was the significant factor. I was led to believe that the expression

$$\pm a \frac{\delta m}{m} \frac{\frac{1}{2}mv^2}{kT}$$

(1) $$e = \epsilon$$

would give the magnitude of enrichment attainable in a process where $\frac{\delta m}{m}$ is the fractional difference in mass of the isotopes being considered, $\frac{1}{2}mv^2$ is the "ordered" kinetic energy, or the kinetic energy ascribed to the average velocity of the particle, $\frac{3}{2}kT$ is the "random" kinetic energy or "thermal" kinetic energy, or the kinetic energy ascribed to the instantaneous differences between the actual velocities of a particle, and that of a particle undergoing only the average motion, and finally, where a is a constant of the order of magnitude of unity.

The formula above may be illustrated by the mechanical centrifuge. Let v_0 be the peripheral velocity at the outer radius R, then the above formula (1) will give $\pm a \frac{\delta m}{m} \frac{\frac{1}{2}mv_0^2}{kT}$

(2) $$e = \epsilon$$

for the enrichment at the periphery over that at the center.

On the other hand, the centrifugal force on a particle at radius r is $\frac{mrv_0^2}{R^2}$, and the integral of the centrifugal force, $W = \int_0^r \frac{mrv_0^2}{R^2} dr = \frac{1}{2}mv_0^2$. Hence, by Boltzmann's relation,

$$(3) \qquad \epsilon = \epsilon^{-\frac{W}{kT}} = \epsilon^{-\frac{\frac{1}{2}mv_0^2}{kT}}$$

The formula may also be readily illustrated by the magnetic mass spectroscope.

Now by purely mechanical or thermal processes, including diffusion, it is possible to give particles ordered kinetic energies of the same order of magnitude as the random or thermal energy. But if the particles can be charged electrically, then with electric and magnetic fields it is possible to give the particles ordered energies thousands of times as large as the random or thermal energies. Hence it would seem that the electromagnetic method of separation should be superior to all others, provided the cost is not too great for producing quantities of ions at low random or thermal energies.

III. Production of Ions at Electrodes of a Vacuum Arc

I had known for some time that the vapor issuing from the cathode spot of an arc between metal electrodes drawn in a vacuum is in a highly energetic state, and therefore probably nearly one hundred per cent ionized. See the following references:

R. Tanberg, Phys. Rev. 35, 1080, 1930
E. Kobel " " 36, 1636, 1930
R. C. Mason & W. E. Berkey, Phys. Rev. 38, 943, 1931

Thus it seemed that so simple a structure as a pair of uranium electrodes carrying an electric arc would be a copious source of uranium ions. Calculations based on earlier data with copper arcs indicated that the power cost would be of the order of 100 kw hrs. per kilogram of ions.

A large part of the time in 1942 was spent in trying to realize this simple ion source.

IV. 1942 Theory of the Ionic Centrifuge

To cooperate with the ion source described above, which would be essentially a point source of ions, I conceived the cylindrically symmetrical ionic centrifuge. Before describing this conception in more detail, let us review the motion which a charged particle would acquire, which originates at a central point with a small initial velocity, and is subjected to a cylindrically symmetrical radial electric field, $E(r)$ with potential $V(r)$, and an axial magnetic field H. The nature of the motion comes out at once by the simple application of the principles of the conservation of angular momentum, and the conservation of energy.

The first principle gives

$$(4) \quad \frac{d}{dt}\left(m r v_\theta\right) = -e H r v_r = -e H r \frac{dr}{dt}$$

Integrating, and assuming that the particle starts at $r = 0$,

$$(5) \quad v_\theta = -\frac{eH}{2m} r .$$

The circumferential velocity is then independent of the electric field and depends only on the radius which the particle has reached. The particles after leaving the origin accuire an angular velocity $\omega_L = -\frac{eH}{2m}$ about the central axis, which is constant, and which we call the Larmor angular velocity. Thus the swarm of ions leaving the central point has a velocity distribution with a constant curl or rotation, similar in this respect to the velocity distribution of the points of a rotating rigid body. If the radial velocities of the ions are kept small, as was expected to be realized, then the motions of the ions will be very similar to the motions of the molecules of the gas in a mechanical centrifuge, and thus the name ionic centrifuge was suggested. However, with readily obtainable magnetic fields, the Larmor rotational velocity for the uranium ion is 30,000 r.p.s. or more, with radius of the order of a foot or more, whereas with the mechanical centrifuge, 30,000 r.p.m. is barely attainable with such a radius.

The energy of the circumferential motion is

$$(6) \quad \frac{1}{2} m V_\theta^2 = e \cdot \frac{eH^2}{8m} r^2 = -eV_L$$

where we call V_L the Larmor Potential. For the uranium ion,

$$(7) \quad V_L = -\frac{1}{19.6} H^2 r^2 \qquad \text{H in kilogauss, r in cm. } V_L \text{ in volts.}$$

For an electron the Larmor Potential will be $(650)^2$ times as large, but of course of opposite sign.

The principle of energy gives us, if we neglect the initial

energy at the origin,

$$(8)\quad \tfrac{1}{2} m v_r^2 + \tfrac{1}{2} m v_\theta^2 \simeq -eV$$

where V is the electric potential at the point reached by the ion, the potential at the origin being zero. This gives

$$(9)\quad \tfrac{1}{2} m v_r^2 = -e(V - V_L)$$

Thus the particle can go out only if V (negative) exceeds numerically V_L (also negative). If the initial velocity of the ion is not zero, but $(v_r)_0 = \Delta v_r$, then (9) becomes

$$(10)\quad \tfrac{1}{2} m v_r^2 = -e\left(V - \left[V_L + \tfrac{1}{e} m \overline{\Delta v_r}^2\right]\right)$$

The effect of the initial velocity is as if the Larmor Potential of the ion were reduced (numerically) by the energy equivalent of the initial velocity.

Concerning the axial motion of the ions, it is clear from the disposition of the axial magnetic field and radial electric field, that each ion leaving the ion source will retain unchanged whatever initial axial velocity it had at the source. Thus, there will be a diffusion of the ions to the axially bounding electrodes of the centrifuge, and this diffusion rate will be independent of the radial electric fields which are impressed.

Suppose, now, that in an ionic centrifuge, that is a cylindrically symmetrical vacuum vessel with a point ion source on its axis, the radial electric field is kept above the Larmor field up to a certain radius, R, and is less than the Larmor Potential beyond this radius. Then if the individual ions follow paths given by equations (6) and (10), they will go out in spirals up to the radius R. Then, retaining the Larmor angular velocity, they will spiral back toward the center. At any particular radius

there will be ions moving outwards and others moving inwards, the radial velocities being given by the excess of the space potential over the Larmor Potential.

The density of the ions will be inversely as r times the radial velocity, and therefore under these conditions the density will be least where the potential numerically exceeds the Larmor Potential the most. It will become very large, or infinite, at the boundary R, where the potential equals the Larmor, but beyond R, where the potential is less than the Larmor, the density will be zero. This distribution of density will be reflected in the deposit of ions on the axially bounding electrodes of the centrifuge. Thus, to obtain a particular desired distribution of deposit on the axially bounding electrodes, this peculiar control of the space potential would be necessary. To get any deposit at all at any particular radius, the potential would need to exceed the Larmor. Then if the deposit was greater than desired, the space potential would need to be <u>increased</u> (negative) If the deposit was less than desired, the space potential would need to be <u>decreased</u>.

All of the preceding conclusions are based on the assumption that each ion moves in the existing magnetic and average electric fields, as if it were alone, and uninfluenced by the presence and motion of the other ions. However, the peculiar phenomena observed at high beam intensities in the Calutron and Magnetron, made me believe that this would not be true at the densities expected in the Ionic Centrifuge. I looked for an interaction between ions, of some sort. This interaction would not consist of

direct collisions in the sense of kinetic theory, because the expected density of ions was too low to expect such kinetic theory collisions to take place. Nevertheless I expected interactions of some sort with similar effects. That is, I believed that a beam of ions, passing through a cloud of stationary ions, would be quickly scattered. I was strengthened in my belief by the known facts concerning the motion of electrons in a plasma. Langmuir(1) had observed that a beam of electrons projected into a plasma is scattered into a Maxwellian velocity distribution in an extraordinarily short distance. Others have verified this and further

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1. I. Langmuir, Phys. Review 26, 585 (1925)

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investigated the subject. (2)

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2. See R. Rompe and M. Steenbeck, Ergebnisse d. Exacten Wiss. V.18, pp. 257 - 376, (1939)

Let us now, therefore, see what we can learn from equations (6) to (10) if we now assume that ions moving relative to one another interact, exchanging momentum and energy.

We first see that the circumferential velocity of the ions will be unchanged by the ion-ion interaction. For the circumferential velocities of the ions are all the same at any point, whether the ions are moving radially outwards or inwards. Hence, interaction should have no influence on the mean circumferential velocities.

However, according to equations (9) or (10), at any point there

will be two radial velocities. Approximately one-half the ions will be moving outwardly and the other half inwardly with the radial velocity given by (9). With ion-ion interaction, such as I expected, these two radial velocities will be very largely converted into a Maxwellian distribution of velocities with the same mean square value.

Thus the picture of the ion motion becomes changed as far as the radial motion is concerned. While the ion cloud or swarm continues to rotate with the constant Larmor angular velocity, the radial distribution of the ions is governed more by diffusion of ions under Maxwellian radial velocity distributions, than by the motion of two oppositely directed beams of ions with velocities given by (9). If a rather complete Maxwellian radial velocity distribution is obtained, then the radial distribution of density of the ions should be governed by a Boltzmann relation,

$$(11) \qquad n = n_0 \epsilon^{\frac{-e(V-V_L)}{KT}}$$

In any case, the density should now be greatest where the potential exceeds the Larmor potential the most. To obtain a particular desired distribution of deposit on the axially bounding electrodes, the electric field should be controlled in this way: when the deposit is less than desired, the potential should be <u>increased</u> in magnitude; when the deposit is greater than desired, the potential should be <u>decreased</u>.

So far we have not spoken of the motion of the electrons which will need to be present to neutralize most of the space charge of the ions. Actually, these electrons will have a density closely equal to the density

of the ions, the two densities differing only by the small amount necessary to give the small charge density called for by the divergence of the Larmor field. It was expected that these electrons would be supplied by secondary emission from the axially bounding electrodes of the centrifuge.

In a crossed constant electric and magnetic field, it is well known that an electron will move in a direction at right angles to the two fields, and with a mean velocity given by E/H. It is clear that with this velocity, the magnetic reaction eHv just balances the electrical force eE. Superposed on this motion will be a motion in a circle with frequency $\frac{eH}{m}$, the resonant frequency of the electron in the magnetic field. If the energy of the electron is small, this circle will be of small radius.

In the moderate radial electric field of the ionic centrifuge, the electron will move with this velocity E/H in a circular path, about the axis of the centrifuge. At the Larmor field of the ion, this velocity will be one-half the Larmor circumferential velocity of the ion, and in the same direction. The small mass of the electron makes the centrifugal force on the electron in this circular orbit negligible. Superposed on this motion of the electron will be a motion in a very small circle with the resonant frequency of the electron. These results may be deduced more exactly directly from the equations of motion of the electron.

Thus the space-charge reducing electrons in the ionic centrifuge will go around in circles, with zero net radial velocity, provided the electrons do not interact or make collisions with other particles. Of course, in the actual centrifuge, the electrons will make collisions with the molecules of the residual gas, and this will cause the electrons to have a

finite radial velocity. This velocity may be readily estimated. Consider an electron in a crossed constant electric field E, and magnetic field H, and in a gas in which its mobility is K. We readily calculate that its velocity in the direction of the electric field is given by

$$(12) \quad V = \frac{KE}{1 + K^2 H^2 \cdot 10^{-16}}$$

where E is in volts/cm., H in gauss, and K in cm./sec. per Volt./cm. At the residual vacuum expected, K is about 10^{10}, E is of the order of 100, and H, 5000. This will give $V = 4$ cm./sec. This is infinitesimal compared with the radial velocity of the ion, about 10^4 cm. per sec. Thus the radial current inward of the space charge neutralizing electrons will be an extremely small fraction of the ion current.

We are now ready to discuss the expected operation of the ionic centrifuge. As already indicated, and as illustrated in Figure 1, the ionic centrifuge is a cylindrical vacuum tank in an axial magnetic field. In the axis of the tank is an arc source of uranium ions. At each axial boundary of the vacuum space is a nest of concentric flat ring electrodes, which can be energized electrically as desired. The outer radius of the vacuum space is bounded by a metallic cylinder, which can also be energized electrically as desired.

What will happen if we raise the potential of say the first end rings negatively, relative to the arc, but keep the other rings at zero potential? There will be a small electron emission from these first rings, photoelectric or otherwise. These electrons will flow out into the space

opposite the first rings, and because of the very small radial mobility of the electron, will raise the potential of this space negatively. Of course, this space potential cannot become more negative than the rings, for then the electrons will flow freely axially back to the rings. How closely the space will follow the negative potential of the rings will depend on the intensity of the electron emission from the rings, but in any case the potential of the space will rise negatively with the rings.

Until the space potential at the inner edge of the rings reaches the Larmor Potential, according to the theory just given, no ions from the arc will reach the rings. The current from the rings will then be just the small electron emission current from them or less. When, however, the space potential becomes greater than the Larmor, ions from the arc will reach the rings and the current taken by the rings may be expected to rise sharply. The ions will not pass beyond the first rings because the space potential is less than the Larmor there. As the first rings are raised negatively high above the Larmor Potential, all the ions emitted from the arc should reach these rings, and the current to these rings should show saturation, but modified somewhat by the small electron drift current.

Now suppose additional rings are raised in potential, negatively above their Larmor Potential. Then they will share in the ion current emitted by the arc. The distribution of current to the rings will be determined by the potentials of the rings. In general, those rings whose potentials exceed their Larmor Potential by the greatest amount will receive the larger fraction of the ions.

However, because of the diffusive character of the radial ion flow, the inner rings will be favored over the outer rings. For example,

to obtain a uniform density of ion current to the excited rings, the outer rings would need to exceed their Larmor Potential by a little more than for the inner rings.

Now suppose, for example, that the rings up to a certain radius are controlled by suitable circuits so that the current density received by the rings is the same for all the excited rings. Suppose also that this current density is chosen so that the total current which this current density corresponds to, is somewhat less than the ion emission current of the arc. Suppose now, also, that the ions emitted by the arc consist of two isotopes. Then if a space potential is considerably above the Larmor Potential for the heavier ion, it will be much less above the Larmor Potential for the other ion. Thus with the given space potential distribution near the Larmor Potential, the ion-current distribution to the excited rings will be very different for the two isotopes.

If the Maxwellian radial velocity distribution is sufficiently well developed so that we may use the Boltzmann relation (11) for the radial density distribution of the one ion, the radial density distribution for the other isotope ion of mass $m' = m + \Delta m$, and Larmor Potential $V_L' = V_L + \Delta V_L$ will be

$$(13) \qquad n' = n_0' \, \epsilon^{\frac{e(V_L - V_L')}{KT}}$$

leading to

$$(14) \qquad \frac{n'}{n_0'} \Big/ \frac{n}{n_0} = \epsilon^{\frac{e\Delta V_L}{KT}} = \epsilon^{\frac{-eV_L \frac{\Delta m}{m}}{KT}} = \epsilon^{\frac{e^2 h^2 r^2 \frac{\Delta m}{m}}{KT}}$$

Thus the degree of enhancement or isotope enrichment which will be obtained will depend on how small KT ~~could~~ can be kept relative to $e\Delta V_L$.

It was believed that KT would be of the same order as given by equation (9) so that by keeping V close to V_L, KT would be kept small. The manner of electrical excitation described above should ensure that $V-V_L$ should be small, since just enough potential would be applied to the rings to bring the ions out at the desired density, and higher potentials than necessary would not be used because the total current demanded was less than the saturation current from the ion source.

It was hoped that KT could be made so small as to be equal to $-e\Delta V_L$, in which case the enhancement would be

$$(15) \quad \frac{n'}{n} \Big/ \frac{n'_0}{n_0} = \epsilon^{+1}$$

which would be a large enough effect to make the ionic centrifuge a very superior device.

I must emphasize in this closing paragraph of this section, that the theory given above is the theory as developed in the first half of 1942. The results talked about are the expected results, and not the actual results obtained in the experiments finally made on an actual centrifuge tested in the last months of 1942, which will be described in later sections.

V. Expected Superiority of the Ionic Centrifuge over other Electromagnetic Devices.

The Ionic Centrifuge was expected to be superior to other electromagnetic separation methods in the following respects.

1. The ion cource is of supreme simplicity, and was expected to give an enormous yield at high efficiency. These expectations were based on previous experience with copper arcs in high vacuum.

Actually, as is described in a later section, difficulty was experienced due to the instability of the arc using uranium electrodes. This difficulty was only partly overcome during 1942. I might say, here, that this difficulty was completely overcome in later work at Westinghouse, where simple arc-ion sources giving many amperes of uranium ions at high efficiency, are in regular use.

2. As used in the ionic centrifuge, the ions are not drawn from the arc and immediately accelerated to high kinetic energies. The acquiring of high kinetic energy is done over a considerable distance, and without intervening electrode structure. Thus, no complicated, and rapidly disintegrating, accelerating electrodes close to the arc are used.

3. In the Ionic Centrifuge, the complete radial output of the arc-ion source is used. In other devices, such as the Calutron, the output is limited to what may be drawn through a slit embracing a very small fraction of 360 degrees about the arc.

4. In the Ionic Centrifuge there is no focussing in the sense as is done in the Calutron or Isotron. The separating effect is permitted to take place at all radii, and throughout the vacuum space. There is thus no high concentration of ions piled up at a receiving slit (Calutron) or receiving plane (Isotron) where intensified interaction effects might be expected to limit the yield at which the device operates properly.

5. The Ionic Centrifuge is expected to operate even with ion-ion interaction. Such interaction would make the Calutron, and perhaps the Isotron, inoperative.

6. The Ionic Centrifuge was expected to operate at a much lower enhancement (of the order of 2) than the Calutron, but at a much lower

voltage, that is, at a few hundred volts as against more than 25,000. At the same time, the drain, or current taken by the power source was expected to be the same as the current of ions separated, whereas for the Calutron this drain was more than ten times the ion beam current. Thus the electrical power requirements for the Ionic Centrifuge per unit effect were expected to be much smaller than for the Calutron.

The lower voltage used by the Ionic Centrifuge, with greatly reduced sputtering effects, also would have many other advantages. At the lower voltage, the cost of power is very much lower per kilowatt hour than at the higher voltage. Also the electrodes used would have an extremely long life, and the recovery of the material would be very much easier.

7. The control of the ionic centrifuge was expected to be very much easier than for the Calutron or Isotron. Simple constant current currents were to be used, with the potentials inherently automatically adjusting themselves to the proper values by the intrinsic properties of the Ionic Centrifuge itself.

8. The ion source in the ionic centrifuge was expected to be of very high intensity. At the same time, since interaction between ions is permitted in the ionic centrifuge, in fact is expected, it was believed that the ionic centrifuge would operate with very large yield.

9. The chemistry of the ionic centrifuge was expected to be very simple. The deposit would be in the form of the metal or oxide, which would be stable and non-corrosive.

10. Let me emphasize that the analysis in this section is as it looked in 1942.

V. Experimental Development of the Arc Ion Source

The ion source in the Ionic Centrifuge was expected to be simply an arc drawn between two uranium electrodes in the axis. Almost at once, as soon as we attempted to put this idea into practice, we ran into difficulties due to the instability of the arc.

I was aware that arcs between metallic electrodes in vacuum were extremely unstable with only a few amperes, but my past experience had indicated that there should be no difficulty when more than ten or twenty amperes were used. However, my previous experience had not included arcs at such high vacuua as we were now using, and also my previous experience involved arcs of short time duration as in vacuum switches.

In February, 1942, our experiments with uranium electrodes quickly revealed the seriousness of this instability.

Starting with a pair of electrodes which had been exposed to the atmosphere, and after pumping down to a high vacuum, a five ampere arc in a 110 volt d.c. circuit could be readily drawn. It would burn for ten or twenty seconds, and then would go out. It could then be restruck, and after a few seconds would again go out. After a few such restrikings, the arc became so unstable that it would go out immediately after being drawn. The oscilloscope indicated that the arc would burn only a few thousandths of a second after being drawn. If the electrodes were exposed to the air again for a few hours, the initial rather limited arc stability would be again evident, but soon it was lost again.

Going to higher current, or a higher voltage circuit, made little change in the performance. The initial arc stability lasted a

little longer perhaps, but soon again arcs of only extremely short duration could be drawn. A large reactor in the circuit did not help. The high voltage, (10,000 to 20,000 volts) developed across the reactor when the arc went out, indicated the hopelessness of this direction of attack.

The effect of degree of vacuum on the arc stability was studied. In helium the arc remained unstable up to 0.2 mm. pressure, but in air it became quite stable at 0.1 mm. This seemed to be too high a pressure for proper operation in an ionic centrifuge.

At first it was thought that this great instability of the arc was a peculiar property of pure uranium metal. However, a few tests showed that copper and iron also showed this instability under similar circumstances.

It was noted that while complete stability was not obtained with air at pressures below .01 mm., nevertheless the duration of the arcing period after each electrode separation was increased by the presence of a little air or oxygen. Experiment indicated that with a few microns of oxygen, the arc duration would be .05 seconds or more. This suggested a vibrating contact arc in the presence of a small amount of oxygen.

A vibrating arrangement was therefore made whereby electrodes could be put into contact and withdrawn about thirty times a second. One electrode was made of carbon with a hole drilled into it lengthwise, through which oxygen could be leaked at a slow rate. The other electrode was, of course, uranium metal. With this structure, and with an oxygen pressure of one or two microns, the arc would burn through most of the 1/60 of a second open period duration of the vibrating contacts. An

average arc current of one ampere, actually two amperes during the arcing period, could be maintained fairly steadily. Currents to surrounding negatively charged electrodes in the vacuum vessel of more than 20 m.a. were observed, indicating that the ion emission was of this magnitude. The polarity of the arc did not make much difference.

All these experiments were made with no magnetic field impressed. When finally the 37" cyclotron magnet became available for a few days test and collection run, it was found that in the magnetic field the arc became increasingly unstable, the arc burning for only one or two thousandths of a second.

Thus, it became increasingly evident to us that vibrating contacts were not the practical answer to the arc stability problem. After the large ionic centrifuge was built and placed in the fringing field of the 184 inch cyclotron magnet, most of the summer was spent trying to find other ways of making the arc stable.

A great variety of experiments were tried, using cooperating carbon electrodes impregnated with various materials. Also additions of various metals were made to the uranium. Results are recorded in the laboratory record books, but are too varied and numerous to record here. In general these experiments were unsuccessful.

It had been reported by others on the project that an arc had been successfully run in a high vacuum between a thermionic filament cathode and a uranium anode. A very high ion emission was also reported. Therefore, a thermionic cathode was constructed to fit into our ionic centrifuge, and

tests with it and uranium anodes were carried out.

It was found, however, that the arc was unstable in the high vacuum. The arc would be started by introducing hydrogen into the vessel until a pressure was reached at which a thermionic arc would start. When the hydrogen was pumped out, the arc would continue to play for a while, but generally, in a very short time, the arc would go out. While the arc lasted, a molten pool would form on the uranium anode, and in this condition the ion emission was quite low.

If a uranium anode was used which had previously been exposed to the air for a long time, the arc, usually of about 5 amperes, would persist for some time, a matter of a few minutes, after the hydrogen was pumped out. Then the arc would terminate on the anode in a bright small spot, with an arc drop of only 18 to 20 volts. Under this condition there would be a large ion emission, usually more than 20 and sometimes up to 200 m.a. But presently a molten pool would form, the ion emission would fall to a low value, the arc drop would rise to 70 to 100 volts and then the arc would go out.

As before, permitting a little air or oxygen to enter the tank would make the arc more stable, but results were rather erratic in this respect.

Again a large variety of experiments were tried with various materials added to the uranium to try to obtain a stable arc with large ion emission. These are recorded in the laboratory record books.

The best result was obtained by using a slab of uranium clamped

to a slab of uranium oxide, and placed so that the electron stream from the filament flowing parallel to the magnetic field would impinge on the junction of the metal and oxide. A bright anode spot would form there, and the arc drop would be less than twenty volts. A large and quite steady ion emission existed in this condition. At a particular anode spot, the ion emission would gradually fall and the arc voltage would rise after a considerable number of minutes. The filament cathode would then be moved slightly and a new anode spot would be formed. The arc current in these experiments was usually five amperes and sometimes ten amperes. The ion emission at a fresh anode spot would be nearly 100 m.a. This type of arc was used in ~~the~~ various final collection runs with the ionic centrifuge.

VI. Experiments with the Ionic Centrifuge

A small ionic centrifuge which would fit into the 37" cyclotron magnet was constructed in the spring of 1942. Using the vibrating type of arc, some observations of its performance were made, and a collection run was also made.

The results obtained conformed with the theory in some ways, but disagreed violently with it in other ways. For example, if the innermost ring was held at zero potential, the current to the outer rings would stay zero, even though their potentials were raised to high negative values. On the otherhand, when the inner rings were made negative in potential, positive (ion) current would flow to rings whose potential was considerably less than the Larmor in value. Also, negative (electron) currents would be found flowing in considerable magnitude to some outer rings.

Some of the samples obtained in the collection run were reported by the chemists to be somewhat abnormal in isotopic constitution. However, at that time, the counting method of analyses, which was used on these samples was just being developed, and there was much uncertainty as to the reliability of these results from the chemists.

Since the 184" cyclotron magnet was soon to be available with a space under the pole face assigned to us, it seemed best to drop the work on the small centrifuge, and to concentrate on the construction of a large centrifuge and its associated circuits, for our further research.

A sketch of the ionic centrifuge designed by Dr. Brubaker and built is shown in Figure 1. Figures 2 and 3 show the location in the magnetic field relating to the experimental Calutron. An electronic power

power supply was also designed by Dr. Brubaker, and built, which permitted continuously adjustable voltages to be applied to each of the twenty-eight ring electrodes of the centrifuge. Later, Dr. Brubaker added elements to the power supply so that it would supply continuously adjustable constant current to each of the twenty-eight electrodes. This apparatus began to be used in the big magnet about the middle of July, 1942.

As with the smaller centrifuge, the currents observed flowing to the various rings as the voltages were varied, contradicted the theory developed in Section IV. While holding the inner rings at zero potential would cut off the flow of current from the arc to the outer rings, nevertheless, current would reach the outer rings when they were at much less than their Larmor Potentials provided only that the inner rings were also made negative in potential.

Quite distressing was the frequent observation of negative (electron) currents to rings. For example, if the rings were given a continuously rising distribution of potentials, as for example, the Larmor distribution, and then if two adjacent rings were adjusted to the same potential, the inner ring would receive positive current, and the outer ring would receive a smaller but comparable negative current. This apparently rather free flow of electrons radially in the centrifuge is completely opposite to the expectations described in Section IV.

Capping the climax may be described results obtained when positive voltages were impressed on the rings as in run 21. It was intended in this run to hold the rings at a moderate positive voltage, about 250 volts, so as to make it impossible for ions to leave the arc, and reach the rings. In this way we expected to get information concerning the quantity

and distribution of neutral molecules leaving the arc. The electron current to the rings was also expected to be practically zero, according to the theory of Section IV. Instead, currents such as follows were observed.

Ring No.	2	3	4	5	6	7	8	9	10	11
Ring Volts	+140	+213	+214	+213	+213	+230	+230	+230	+230	+230
Lower Ring Current, m.a.	-80	-17	+1.2	+0.8	+0.3	+0.2	-0.3	-0.2	0	0

Thus very large electron currents seem to flow to the inner rings, and at the same time, against a strong opposing potential, positive ion currents reach outer rings. Measurements were also made of the deposited material, which indicated that more material came out to the outer rings when the rings were made positive than when the rings were held at zero potential.

These results were quite incomprehensible. It was only in later years at Westinghouse that it became clear how they might be accounted for by an interaction between ions and electrons.

Nevertheless, the experiments showed that in the magnetic field, currents were drawn from the arc to ~~the~~ energised. rings with hundreds of volts. This could only mean that the current carrying particles must acquire corresponding kinetic energies. In the magnetic field, there did not seem to be any doubt that it would be the positive ions which would acquire this energy rather than the electrons. Also, with direct, kinetic theory collisions between ions and electrons or molecules negligible in numbers, it was hard to see how the kinetic energy of the ion (presumably that of some oscillation) could be quickly dissipated into random or thermal energy, and therefore,

presumably, the energy of the ions would be of ordered form.

It seemed, therefore, that while the ions (and electrons) do not move in the manner expected by the theory of Section IV, nevertheless, the ions do acquire a high ratio of ordered to random kinetic energy, and it should be possible to effect a separation of isotopes through this motion.

At the time these results were found, i.e., in the fall of 1942, the space under the magnet poles was believed to be available only for a few weeks more, and therefore, it seemed that the best thing we could do would be to make a few collection runs, and see what isotope separations could be found. Of course, the running would be rather blind, as the only guide we had as to the proper voltages or currents to apply to the electrodes was the theory of Section IV, which experiment had shown to be incorrect.

As it turned out, more time was available than we anticipated, and we were able to make a large number of collection runs before the work in Berkeley was stopped in ~~December~~ November, 1942. The results obtained will be discussed in Section VIII, as it will be necessary to first discuss, in Section VII, the method of isotope analysis used.

We close this Section by describing to what extent the deposited material conformed to the pattern of currents supplied to the rings. As an example, in Run 13, rings 2, 3, 4, 5, 6, and 7 were excited each from a constant current circuit, and the currents were so chosen that each ring pair received the same current density as the others, and the total current was 5 m.a. The rings beyond 7 were allowed to float electrically. Collectors

of ~~one~~ two square inch area were fastened to the various rings. After a run of 2½ hours, with a magnet field current of 650 amps., and pressure averaging 1.4×10^{-4} mm., the relative densities deposited on the collectors was obtained by taking alpha counts with each collector.

The results are shown in Figure 4. The deposit on the lower rings was heavier than on the upper because the arc was set near the bottom of the tank, only six inches from the lower rings. On adding the deposits on lowers and uppers together, we get a gratifyingly constant deposit on rings 2 to 6, but ring 7 which should also show this same deposit, has considerably less. The deposit should drop sharply to zero beyond ring 7 according to the theory of Section IV, but instead, it drops off in a rather gradual manner. An enrichment factor of 1.14 was indicated by the chemists for the sample taken from ring 3, lower, but there are uncertainties about this as described in the next section.

An example of a less favorable looking run is shown in Figure 5. Here rings 2 to 16 were excited with uniform current density with a total current of ~~10~~ 14 m.a., pressure 1.10^{-4} mm. and magnetic field current of 650 amperes. Running time 3½ hours. The α counts were converted to micrograms per in.^{2} by the appropriate multiplier. In this run the chemists did not report results indicating any large enrichment factor.

VII. The Isotope Analyses as Made at Berkeley

Before giving the isotope separation results of the collection runs made with the ionic centrifuge, it is necessary to discuss the isotope analytical methods used by the chemists, as the reports of the chemists were contradictory, and it is necessary to understand the method used to see that certain of the results were more credible than the others.

The method used was developed and supervised by Drs. Segre and Kennedy. Subsequently it was developed into an accurate and dependable method, but at this time in 1942 there were still unrecognized sources of error. The method is as follows. The material of a specimen is dissolved and purified, and a small amount is deposited as a thin film of oxide on a platinum surface. The mass, m, of the film is determined by weighing on a micro-balance. The α-activity, α, and the fission activity, f, are determined by a suitable counting apparatus and a monitored neutron source. From these data the ratio of the quantities α/f, α/m, f/m to the corresponding quantities for normal material may be obtained. In what follows, let the symbols (α/f), (α/m), (f/m) stand for these ratios.

Only two of these ratios are independent, as the third can be calculated from the other two. From any two of these ratios, the isotopic constitution of the film may be calculated, as follows.

The fission activity of the film is given entirely by the activity of the 235 isotope. Assuming that the amount of 235 isotope is small compared to the amount of 238 isotope, we have

$$(16) \quad e_{235} = \left(\frac{n_{235}}{n_{238}}\right)_{sample} \Big/ \left(\frac{n_{235}}{n_{238}}\right)_{normal} = \left(\frac{f}{m}\right)$$

The α activity, however, is due half to the activity of the 238 isotope, and half to the 234 isotope. Again assuming that the amount of 234 isotope is small compared to the amount of 238 isotope, we have

$$(17)\quad e_{234} = \left(\frac{n_{234}}{n_{238}}\right)_{sampl} \Big/ \left(\frac{n_{234}}{n_{238}}\right)_{nrmal} = 2\left(\frac{\alpha}{m}\right) - 1$$

Thus (16) and (17) give the isotopic constitution of the film from the two ratios (α/m), and (f/m).

In the case of the ionic centrifuge, however, and particularly with small enrichments, we can go farther and determine the isotopic constitution of the sample from any <u>one</u> of the three ratios (α/m), (f/m), and (α/f). For in the centrifuge, if the 235 isotope is enriched, then so also certainly will be the 234 isotope. Furthermore, since the mass difference for the 234 isotope is 4/3 of the mass difference for the 235 isotope, we may expect that for small enrichments, the 234 enrichment will be correspondingly larger than the 235 enrichment. That is

$$(18)\quad \left(e_{234}-1\right) = \frac{4}{3}\left(e_{235}-1\right)$$

This leads to the following three expressions for the 235 enrichment in the ionic centrifuge.

$$(20)\quad \left(e_{235}-1\right) = 1.5\left[\left(\frac{\alpha}{m}\right) - 1\right]$$

$$(19)\quad \left(e_{235}-1\right) = \left(\frac{f}{m}\right) - 1$$

$$(21)\quad \left(e_{235}-1\right) = \frac{1 - \left(\frac{\alpha}{f}\right)}{\left(\frac{\alpha}{f}\right) - \frac{2}{3}}$$

Thus e_{235} calculated from either (19), (20), or (21) should give the same result.

Unfortunately, as described in the next Section, the results reported by the chemists never met this test of consistency. Formulas (19), (20), and (21) gave widely discordant results.

Study of the results obtained, indicated that probably the most uncertain quantity in the analyses was the determination of the mass, m, of the 238 isotope in the film. This was indicated by the fact that e_{235} calculated from the chemists' (α/f) which did not depend on m, varied far less wildly than e_{235} calculated from the chemists' (α/m) or (f/m). Generally ~~e_{235} determined from~~ (α/m) ~~or~~ and (f/m) ~~was~~ were absurdly small, indicating that the chemists were over-estimating the amount of 238 in the film. Although the samples given to the chemists were greater in amount than they believed necessary for their proper handling, nevertheless, it seems likely that in the purification process some troublesome impurity was left in the sample in sufficient amount to falsify the chemists' determination of the weight of the 238 in the film.

Thus, e_{235} determined from the chemists' (α/f) seemed the most credible estimate of this quantity. However, even this sometimes varied enough in different determinations with the same sample as to leave uncertainties as to the actual magnitude.

VIII. The Collection Runs with the Ionic Centrifuge.

Thirty ~~four~~ three collection runs were made with the ionic centrifuge. Eight were made with inner rings energized at constant current density, and the outer rings floating. ~~Eleven~~ Twelve were made with an outer ring energized at about its Larmor Potential, with all the inner rings floating. Four were made with the rings energized at their estimated Larmor Potentials. ~~Seven~~ Eight were made with the rings all at zero potential. One was made with + 250 volts on the rings.

Many samples were taken from the various runs. Many samples were analyzed by the chemists, but many were not analyzed. The results of the analyses are shown in Figures 6, 7, 8, and 9. In these Figures the ordinates and abscissae are the (α/f) and (f/m) values respectively for each sample as reported by the chemists. There is also shown an ordinate scale giving the enrichment e as calculated from α/f by equation (21). If the work of the chemists was perfectly consistent, then this e calculated from (α/f) would agree with the (f/m) observations, and all the points should lie on a curve, called the "Theoretical Curve" in Figures 6, 7, 8, and 9.

Sometimes, two or three analyses were made on the same sample. In these cases straight lines are shown in the Figures joining the points. An arrow on the straight line indicates the order in which the analyses were made. The material of the sample was re-purified before each new analysis so that presumably each later analysis was more reliable than the earlier one. On Figures 6 and 7 are shown some points enclosed in circles. These

are analyses made at Columbia, which will be described in the next Section.

It will be seen that only a small fraction of the points lie on the "Theoretical Curve." In each Curve, the per cent spread in the (f/m) values is many times the per cent spread in (α/f) values, indicating that the (α/f) values are more reliable than the (f/m) values.

This is particularly true and significant for the successive analyses on the same samples. The following table brings this out.

	First Analysis to Second		Berkeley Analysis to Columbia Analysis	
Sample	% change in(α/f)	% change in (f/m)	% change in (α/f)	% change in (f/m)
Run 13 3L	-7.9%	-4.6%		
Run 15 3L	+3.4%	+45.1%	-4.3%	+23.0%
Run 15 7L	-9.9%	+40.0%		
Run 20 2U	-3.5%	+ 6.5%		
Run 26 2L	-4.2%	+ 2.9%		
Run 10 18U			-4.1%	+55.5%
Run 12 UW	-0.5%	+12.0%		
Run 17 2L	-1.0%	+21.4%		
Run 17, 2L 2nd purification	-3.4%	+ 5.3%		
Run 34 2LB	-1.3%	+ 4.5%		

From the foregoing table we see that the maximum change in (α /f) by repurification was 9.9%, and the average change was 3.9%. It is not implausible to assume that (α/f) is determined with an average error of less than 5%. If we make this assumption, then examination of Figures 6, 7, 8, and 9 show that many samples produced in the ionic centrifuge had e greater than 1.1, and quite a few had e greater than 1.5.

IX. Analyses Made at Columbia

It seemed important to verify in some way whether the large effects indicated by the Berkeley chemists were real or not. The size of the samples taken was too small for mass spectroscopic analysis. I visited Dr. Urey in New York in January 1943, who suggested that perhaps some of the sample films which the Berkeley chemists had found abnormal, might be re-examined by analysts at Columbia, also using the counting method to see how well they would agree. I accordingly sent for the sample films of Run 10, 18U, Run 15, 3L, Run 15, 6L, and Run 15, 7L.

I delivered these films to Dr. Dunning, whose assistant, Dr. Hull, carried out the work. After a period of experimenting and practicing in the delicate microchemical operations involved, Dr. Hull succeeded in determining the isotopic consitution of the films as follows.

Dr. Hull first made α and f counts on the films as received. He then dissolved off the film, and determined its mass. Thus he was able to check the α/m and f/m figures of the Berkeley chemists. Dr. Hull, then purified the dissolved film material, made a new film, and made new α/m and f/m determinations. In the case of the samples Run 15, 6L, and Run 15, 7L, the amounts were too small to work individually, so they were combined together.

These results were reported in March, 1943.

Sample Run 10, 18U

	α/m	f/m	α/f	e by (α/m)	e by (f/m)	e by (α/f)
Berkeley Report	1.30	1.60	0.81	1.45	1.60	2.33
Columbia Check of Berkeley Sample	1.42	1.80	0.79	1.63	1.80	2.70
Columbia Re-purified sample	1.92	2.49	0.78	2.38	2.49	2.94

Sample Run 15, 3L

	α/m	f/m	α/f	e by (α/m)	e by (f/m)	e by (α/f)
Berkeley Report	0.99	1.05	0.94	0.98	1.05	1.22
Columbia Check of Berkeley Sample	1.00	1.03	0.96	1.00	1.03	1.14
Columbia Re-purified sample	1.16	1.29	0.90	1.24	1.29	1.43

Samples, Run 15, 6L, Run 15, 7L

	α/m	f/m	α/f	e by (α/m)	e by (f/m)	e by (α/f)
Berkeley Report 6L	1.09	1.20	0.91	1.13	1.20	1.37
Berkeley Recount 6L	1.11	1.24	0.90	1.16	1.24	1.43
Berkeley Report 7L	0.80	0.88	0.92	0.70	0.88	1.32
Berkeley Repuri-fy, 7L	1.01	1.22	0.83	1.02	1.22	2.04
Columbia 6L + 7L	1.07	1.17	0.92	1.10	1.17	1.32

We see that Dr. Hull confirmed the Berkeley findings that there was a very large enrichment in the samples tested, and Dr. Hull was able to very greatly increase the consistency of the e determinations.

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X. Conclusions and Recommendations as of April, 1943

"I am very happy to report now, that thanks to the efforts of Drs. Dunning and Hull at Columbia, the reality of these high enrichments has been established beyond any reasonable doubt.....

"These figures are so extraordinary that they cannot be lightly dismissed. They show that the ionic centrifuge itself, or else some element or factor which appeared in the ionic centrifuge, has a very powerful separating effect.

"Recommendation.

"A larger volume magnetic field and facilities and personnel for further study of the ionic centrifuge should be provided."

The above quotations from letters in the spring of 1943 sum up the status of the ionic centrifuge at that time.

The "larger volume magnetic field and facilities and personnel" were provided in the summer of 1944 by the Westinghouse Electric Corporation at East Pittsburgh, where work has been continued.

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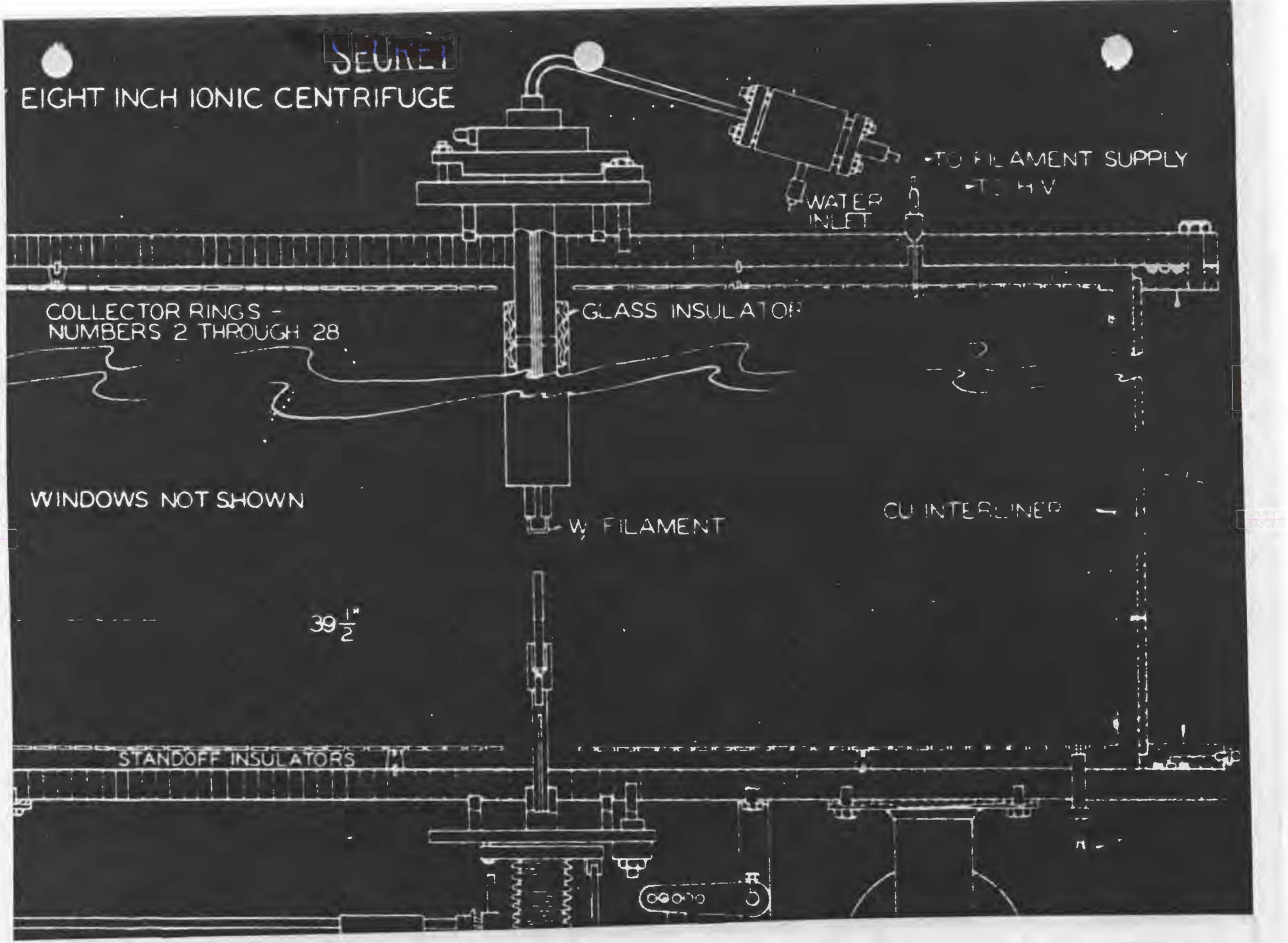
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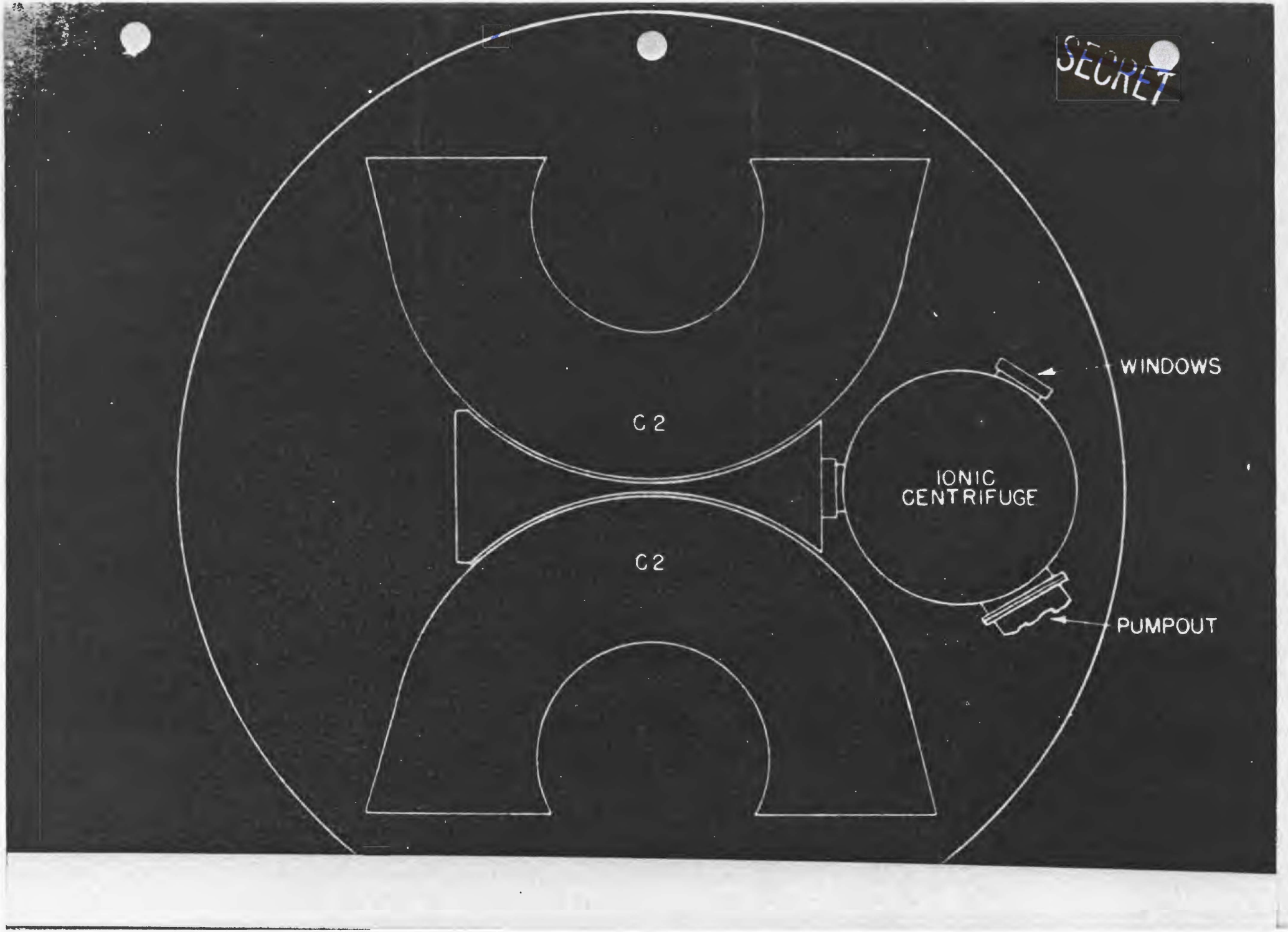
EIGHT INCH IONIC CENTRIFUGE
TO FILAMENT SUPPLY
TO H.V.
WATER INLET
COLLECTOR RINGS - NUMBERS 2 THROUGH 28
GLASS INSULATOR
WINDOWS NOT SHOWN
W FILAMENT
CU INTERLINER
39 1/2"
STANDOFF INSULATORS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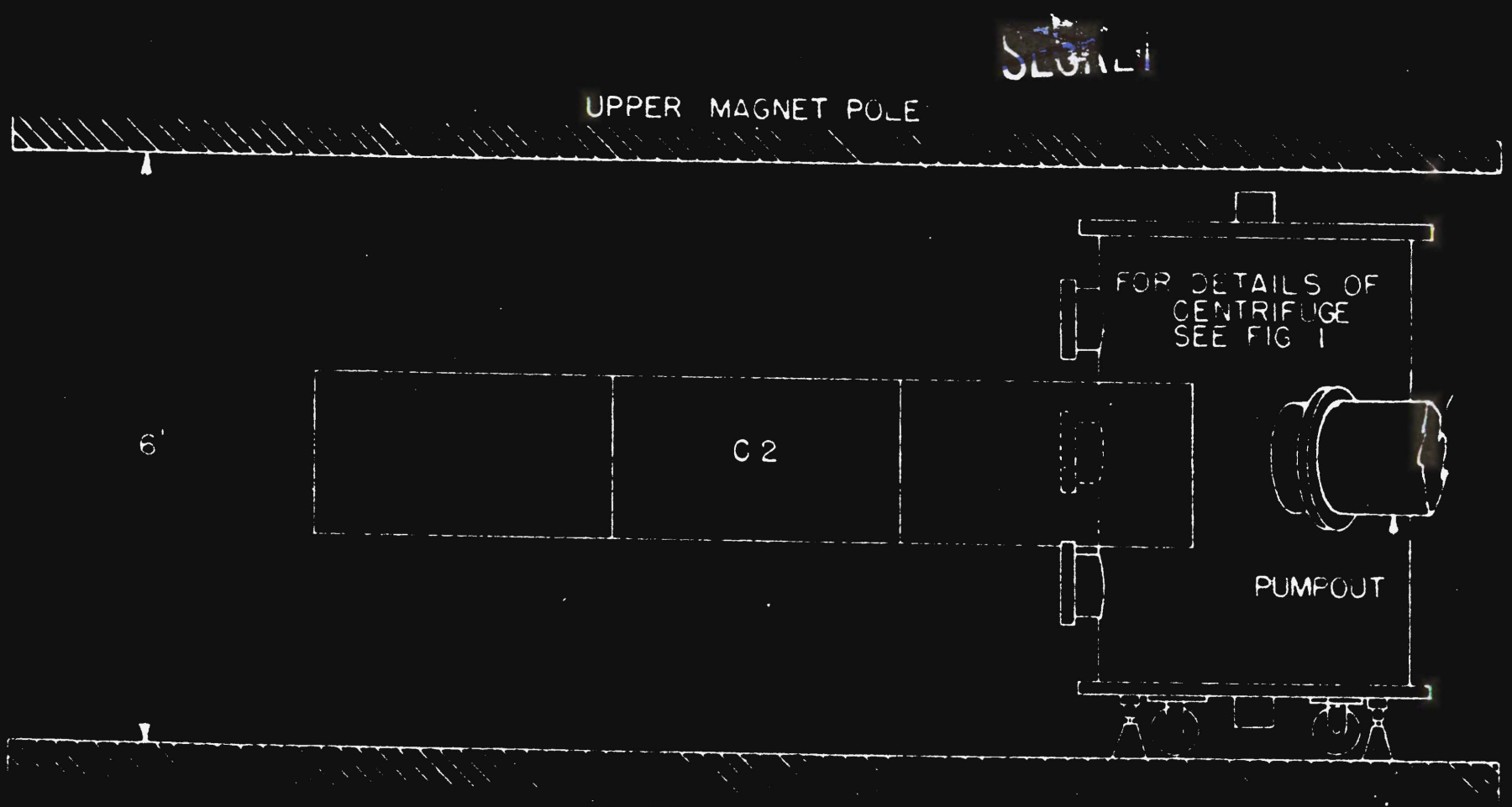

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FIG 3

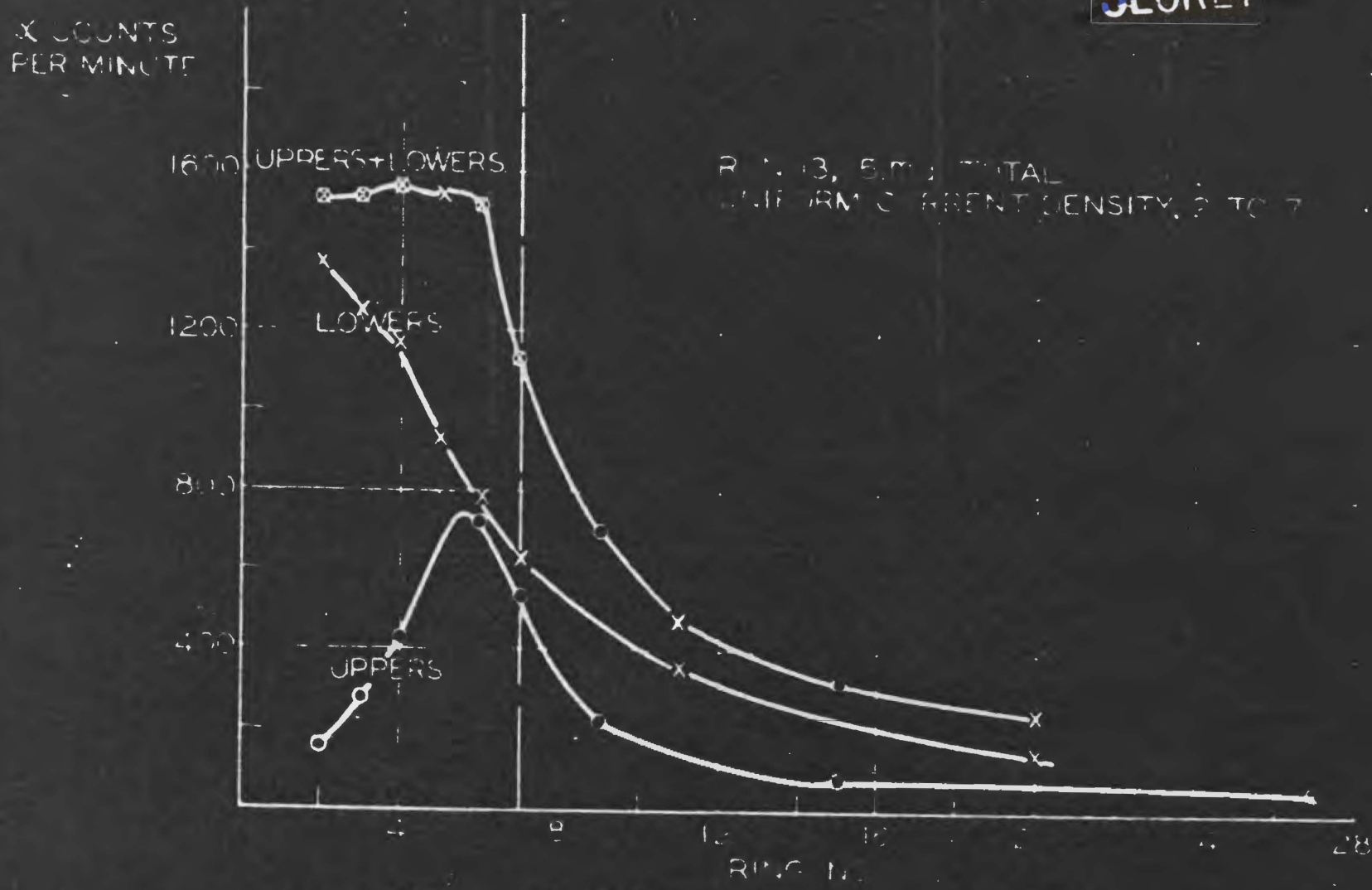

FIG. 4—DISTRIBUTION OF DEPOSIT, RUN 13

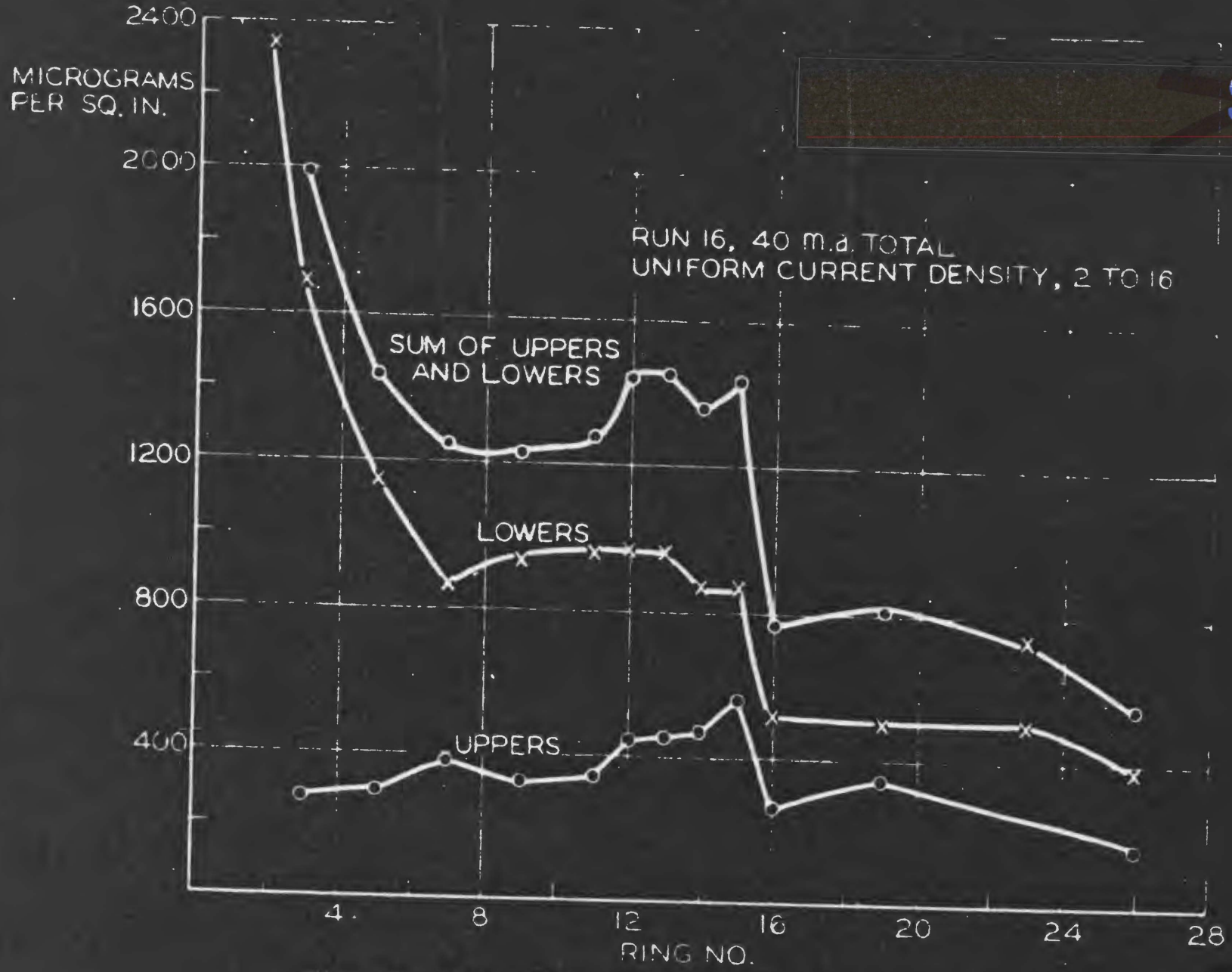

FIG. 5–DISTRIBUTION OF DEPOSIT, RUN 16

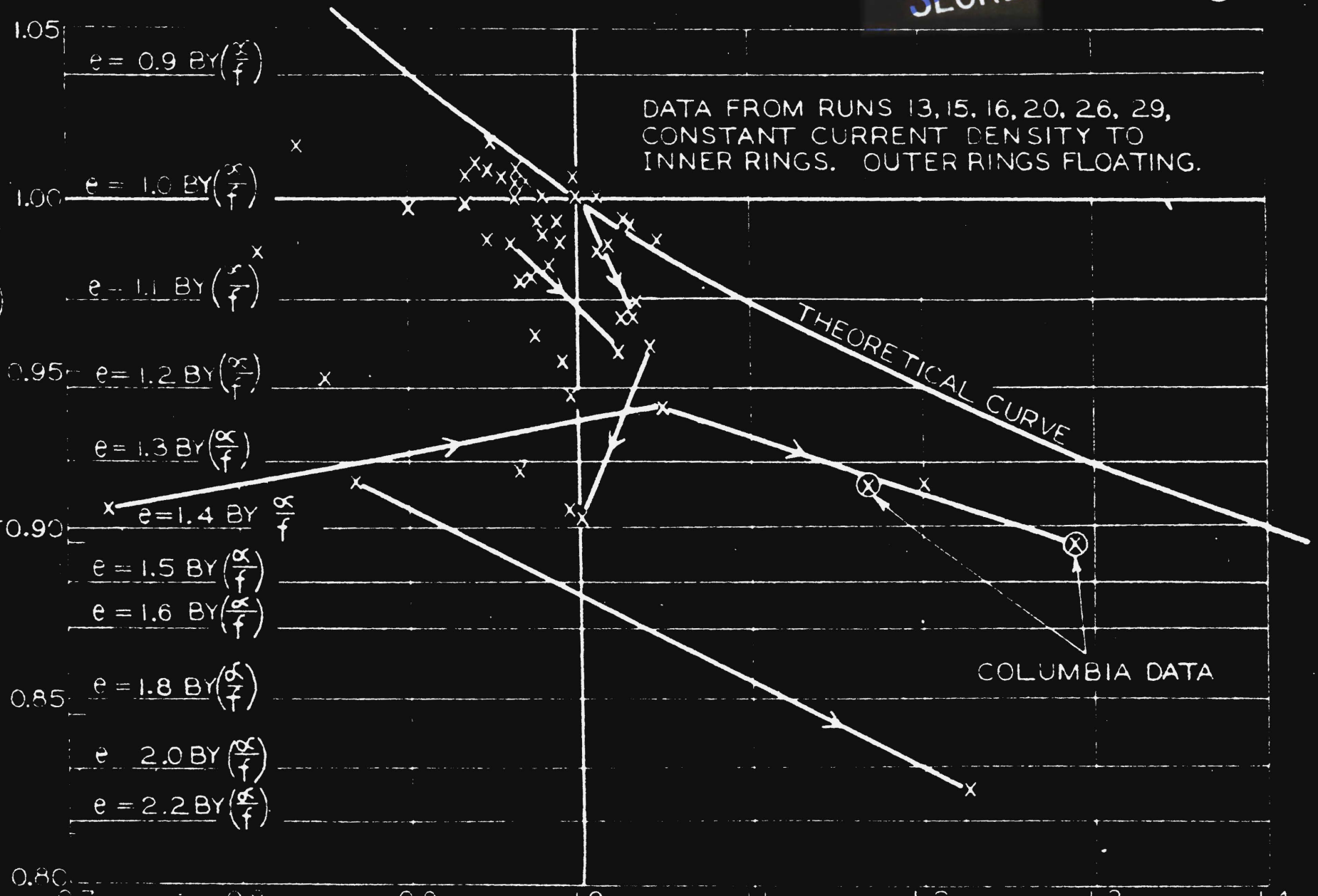

FIG. 6 – COLLECTION RUN DATA

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RESEARCH LABORATORIES

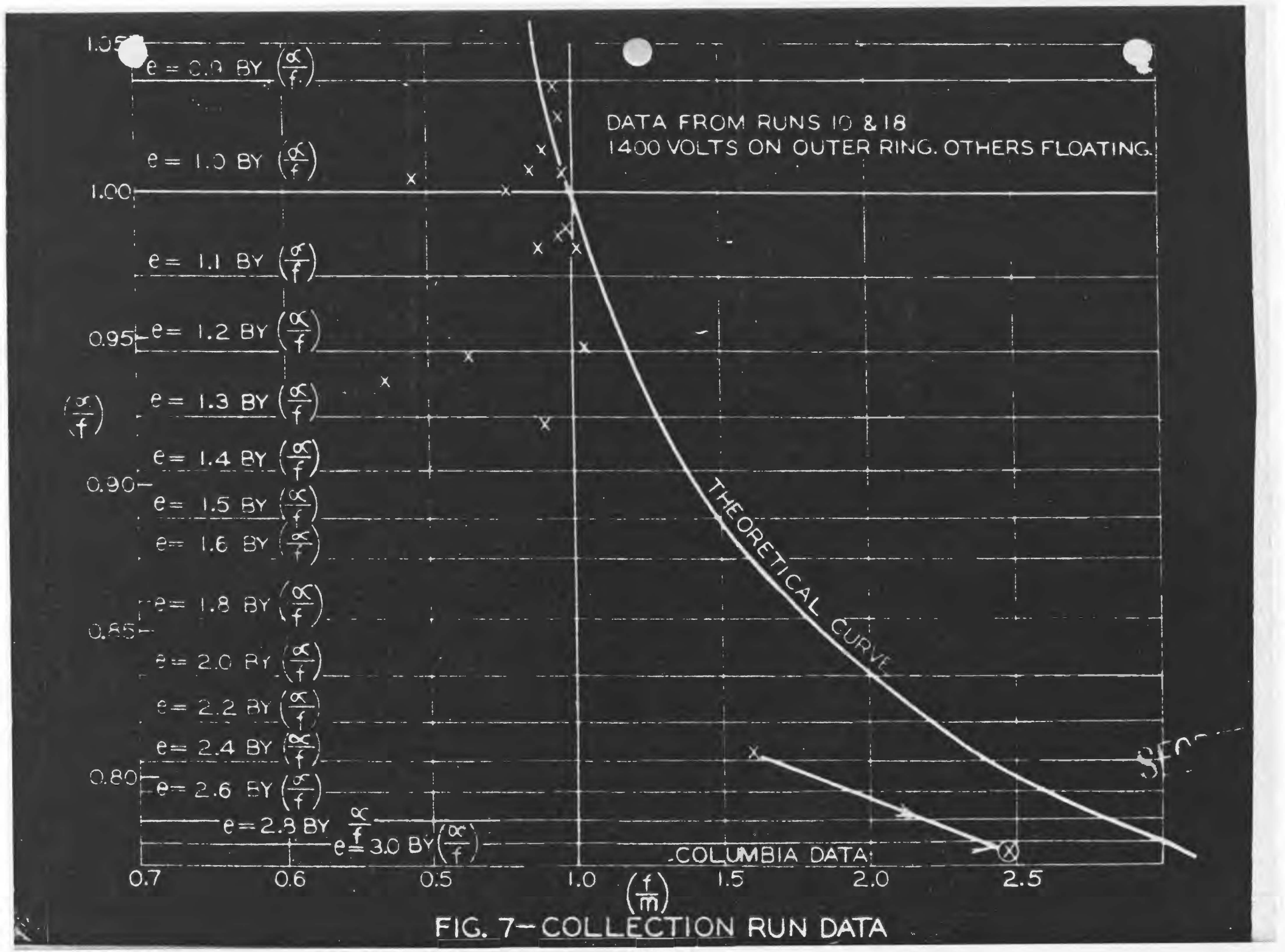

FIG. 7— COLLECTION RUN DATA

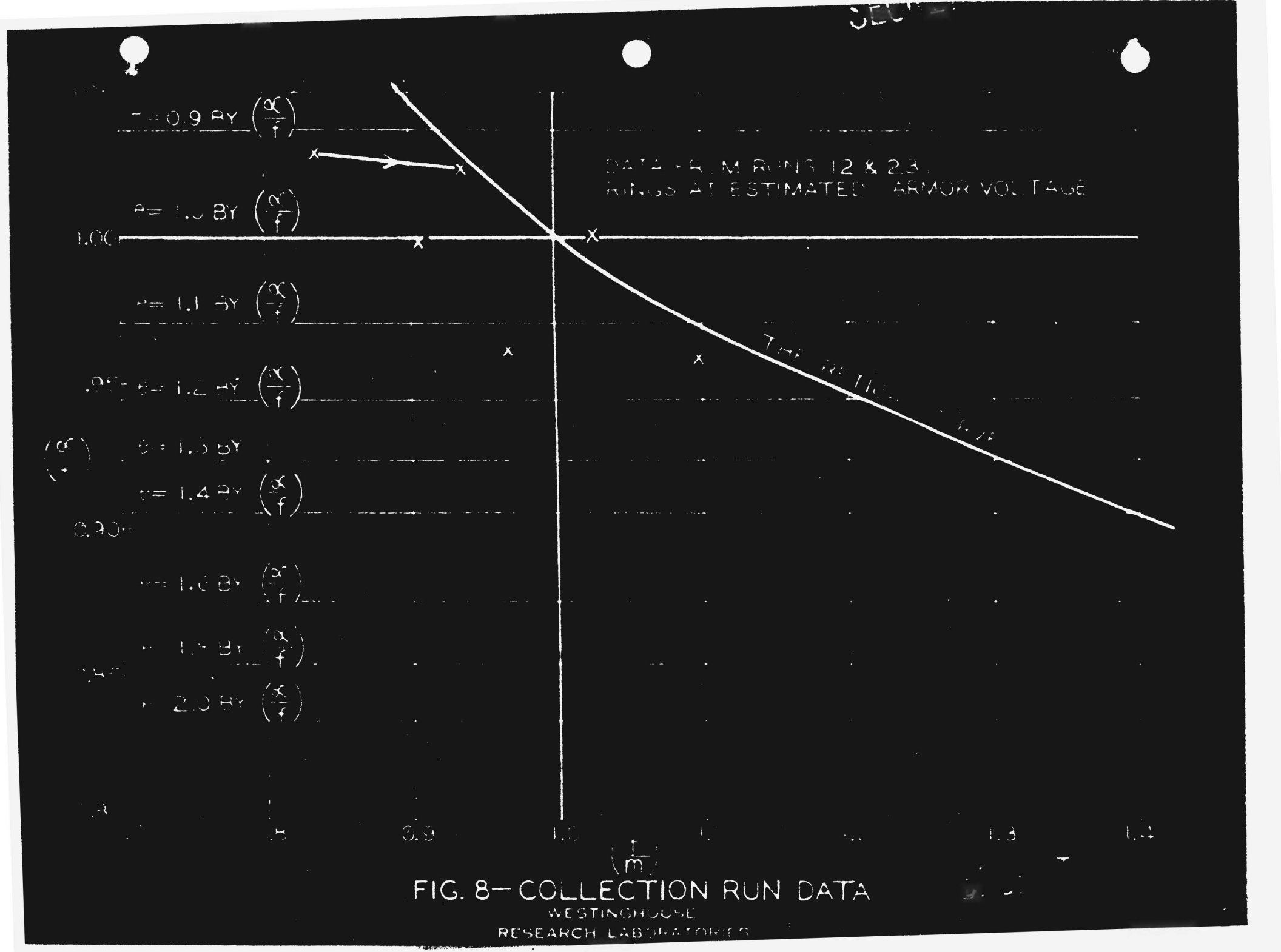

FIG. 8— COLLECTION RUN DATA

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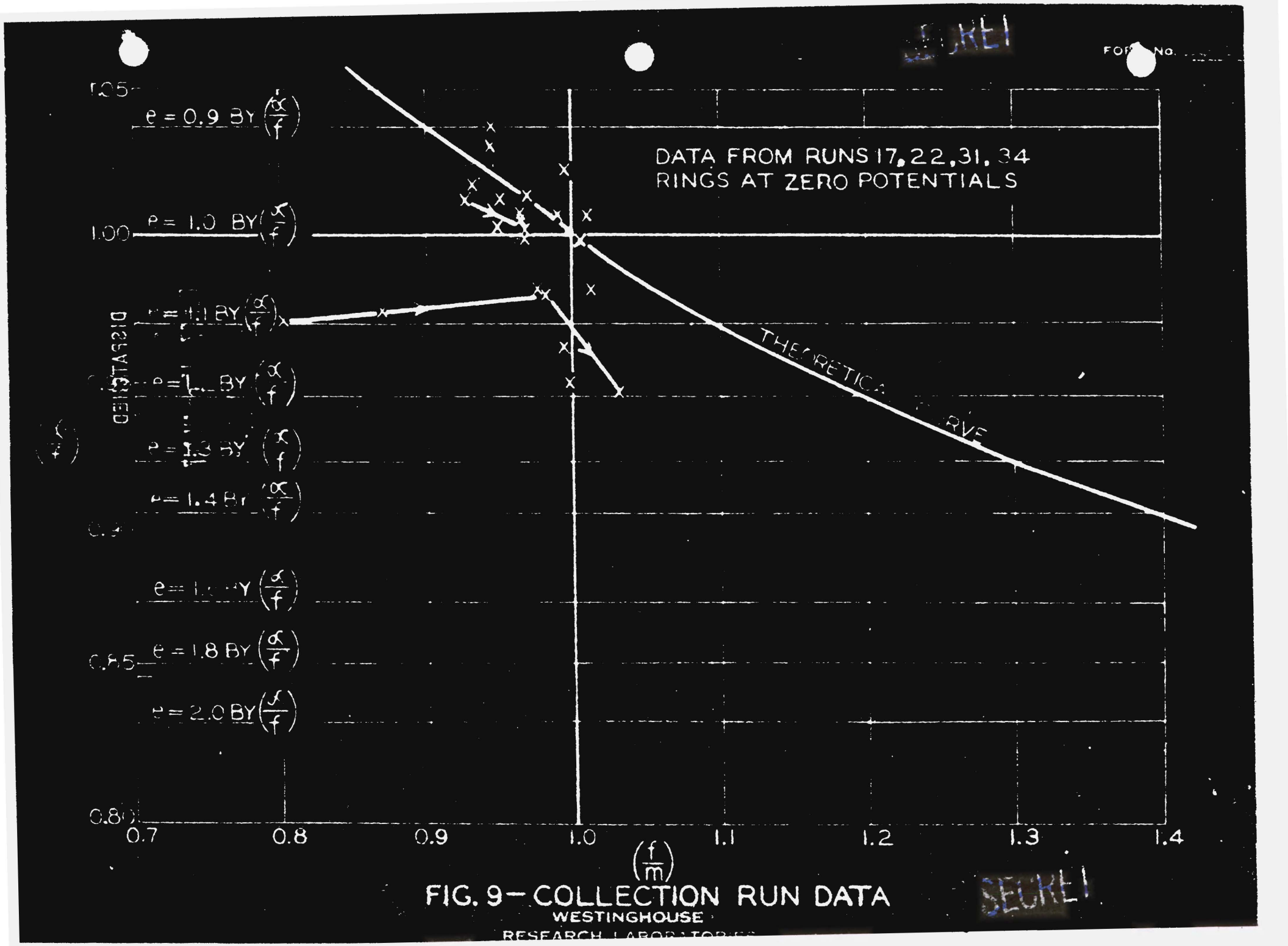

FIG. 9 – COLLECTION RUN DATA

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VOLUME 4 - AUXILIARY ACTIVITIES

CHAPTER 14 - INVESTIGATIONS OF MISCELLANEOUS PROCESSES OF SEPARATION OF URANIUM ISOTOPES

APPENDIX B - REFERENCES

No.	Description	File Location
1	Report (unnumbered) - Centrifuge Method of Separating Isotopes of Uranium - 7 December 1946 - J. W. Beams	AFSWP File
2	Report A-49 - Comparison of Different Methods Applicable to the Separation of Uranium Isotopes - Undated - H. C. Urey	AEC File
3	Report A-50 - Theory of the Simple Process Flow-Through Centrifuge - 6 February 1941 - K. Cohen	AEC File
4	Report A-51 - Thermal Phenomena in Centrifugation - Undated - C. Skarstrom and Cohen	AEC File
5	Report A-52 - The Influence of Baffles on a Counter-Current Ultra-Centrifuge - Undated - K. Cohen	AEC File
6	Report A-53 - Main Numerical Results on the Separation of UF_6 - 3 March 1941 - K. Cohen	AEC File
7	Report A-54 - Concentration of Isotopes by Fractional Distillation in an Ultra-Centrifuge - Undated - K. Cohen	AEC File
8	Report A-101 - Absolute Efficiencies of Isotope Separation by Counter-Current Centrifuges - 28 January 1942 - K. Cohen and I. Kaplan	AEC File
9.	Report A-143 - Progress Report on Isotope Separation - 19 March 1942 - H. C. Urey	AEC File

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No.	Description	File Location
10.	Report (unnumbered) - Time of Equilibrium of a Single Refluxing Centrifuge - 12 June 1942 - I. Kaplan and K. Cohen	AEC File
11	Report A-195 - The Approach to Equilibrium of the Counter-Current Centrifuge - 17 June 1942 - I. Kaplan and K. Cohen	AEC File
12	Report A-302 - Some Properties of Cascades of Concurrent Elements - 19 September 1942 - K. Cohen and I. Kaplan	AEC File
13	Report (unnumbered) - The Westinghouse Gas Separators, Developments OEMsr 429 and 415 - Undated - A. C. Hagg	AFSWP File
14	Report A-1793 - Gas Centrifuge Development Project - 20 March 1944 - Standard Oil Development Company	AEC File
15	Letter and Work Review - Letter from Paul B. Kuhl to E. V. Murphree, dated 2 December 1943, transmitting Summary Review of G.S.P. Plant Operation by B. C. Belden, F. R. Russell and B. G. Gillespie, dated 1 December 1943.	AFSWP File
16	Report A-648 - Princeton University OSRD Project SSRC-5. Overall Report #13. The "Isotron" Method. General Report covering period from December 1, 1941 to May 15, 1942 - Undated - H. D. Smyth and R. R. Wilson	AEC File
17	Report A-803 - Princeton University OSRD Project SSRC-5. The Isotron Method General Report #18 covering period from May 16, 1942 to August 20, 1942 - Undated - H. D. Smyth and R. R. Wilson	AEC File
18	Report A-824 - Memorandum For Members of S-1 Executive Committee, Preparatory to Visit to Princeton - 16 December 1942 - H. D. Smyth	AEC File
19	Report A-824, Suppl. 1 - Memorandum For Members of S-1 Executive Committee - 13 January 1943 - H. D. Smyth	AEC File

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No.	Description	File Location
20	Report A-1742 - Princeton University OSRD Contract OEMsr 297. Final Report - October 1943 - H. D. Smyth	AEC File
21	Report A-713 - Final Report of the Sterling Chemical Laboratory in Fulfillment of Contract OEMsr 381 and Extensions 1, 2, 3 - 19 May 1943 - Herbert S. Harned.	AEC File
22	Wellner, Matchett and Levine - Ind. Eng. Chem., Annual Edition 16, 519 (1944).	General Publication
23	Report DR-512 - Recent Experiments on the Fractional Sublimation Method of Uranium Isotope Separation - 31 July 1945 - Allen F. Reid	AFSWP
24	Report A-1933 - Final Report on Contracts OEMsr-106, OEMsr-107, OEMsr-192, OEMsr-412, and Associated Sub-contracts - 28 February 1944 - Harold C. Urey	AEC File
25	Report A-455 - Final Report of the Pennsylvania Group on Substitute Materials Project, 1 September 1942 to 31 December 1942 - 31 December 1942 - Martin Kilpatrick	AEC File
26	Report A-519 - Final Report on Contract OEMsr-788, Design of Ether-Water Contacting System - duPont de Nemours & Company - 18 January, 1943 - J. B. Tepe and W. K. Wood	AEC File
27	Report (unnumbered) - Spectrographic and Photochemical Properties of Uranium Compounds With Particular Reference to Isotopic Separation - Undated - G. H. Dicke and A. B. F. Duncan	AEC File

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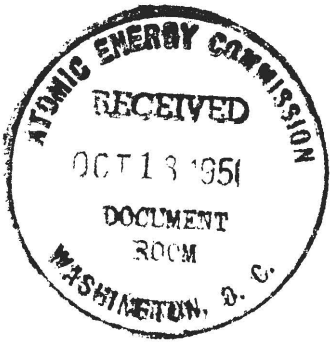
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